THE

Psychological Review

EDITED BY

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WITH THE CO-OPERATION OF

Volume IV. 1897.

PUBLISHED BI-MONTHLY BY
THE MACMILLAN COMPANY,
41 N. QUEEN ST., LANCASTER, PA.
66 FIFTH AVENUE, NEW YORK; AND LONDON.

PRESS OF
THE NEW ERA PRINTING COMPANY,
LANCASTER, PA.

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VOL. IV. No. 6. NOVEMBER, 1897.

THE PSYCHOLOGICAL REVIEW.

STUDIES FROM THE PRINCETON PSYCHOLOGICAL LABORATORY, VI–VII.

VI. THE REACTION TIME OF COUNTING.

BY PROFESSOR H. C. WARREN.
Princeton University.

I. INTRODUCTION.

The problem underlying this study was the question as to how we determine the number of things in a group. The mental process concerned in this determination is evidently not the same as the function technically known to experimental psychologists as *discrimination*. The latter consists in distinguishing between two or more different things; an object is ascertained, by means of certain marks or characteristics, to be the thing sought for and not something else; or the absence of these characteristics is noted and it is thus known not to be the thing sought for. It is also a mental process distinct from *recognition*; we speak (technically) of recognizing an object or objects when we recall their former presence in consciousness by means of certain marks and are thereby able to class them or give them a name. The *knowledge of the number of things in a group*, on the other hand, is independent of marks or differences. Number depends solely on the *distinctness* or *separateness* of the objects; it has nothing to do with their complexion. The word discrimination might readily be applied to the numbering process, and so might the word recognition; but if this were done it could only be through a change from their techni-

cal connotation; 'numbering' is very different from the processes to which these two words are applied by experimentalists; the mark of 'five-ness,' if we may use such a term, is simply the spatial or temporal distinctness of the objects in the group—any or all of the objects can be exchanged for any others, however different, and the 'five-ness' remains unaltered; this does not hold true in the case of ordinary recognition or discrimination.

It is not necessary here to enter into any discussion of the origin of the concepts 'one,' 'two,' 'three,' etc. This is an entirely separate question, which has already received considerable attention from psychologists and mathematicians.[1] In the present study we were concerned solely with the proper application of these terms to given groups of objects. That is, we were to investigate the concrete process of *numbering*, rather than the process of acquiring the abstract *number concepts*.

Whatever the nature of this numbering process, and whatever different kinds of numbering there may be, it is proper enough to denote the function by the term *counting*, as we shall do throughout this paper. But we must distinguish at the outset between several varieties of counting. The most important distinction is that between counting proper and inferential counting. In the former, objects are added up, so to speak, by a sort of mental 'one-two-three-ing;' in the latter, some clue is given by the form of the group, the amount of space it occupies, the amount of time required to survey it, etc.; thus, the familiar quincunx form (⁘) is taken in as a whole—the form of the figure is associated with the number-name, by a mass of former experiences, as firmly as is the symbol '5.' The present study was concerned primarily with the former process; the latter is a species of association or inference (as the case may be), whose investigation involves a different problem; indeed, its chief rôle in our study was that of an enemy to be thwarted at all hazards.

[1] On this point see 'The Number Concept,' by L. L. Conant, New York, Macmillans, 1896, and 'The Number System of Algebra,' by H. B. Fine, Boston, Leach, Shewell & Sanborn, 1897.

A further distinction is to be noted, within the process of counting proper, between that which is practically instantaneous and that which involves the expenditure of time. If it takes no more time to count Three[1] than to count Two or One, it is evident that the apprehension of each separate object does not involve time; if the reaction time of these numbers be practically the same, then their counting proceeds by an apprehension of the group as a whole, rather than by successive apprehension of its members. Whereas, if the reaction time of Four (say) is longer, the increment is time consumed in apprehending the extra unit. We may call these two processes *perceptive* and *progressive* counting, respectively; add to this the process already noted, *inferential* counting, and we have three distinct methods of counting. I give this classification here without discussing its practical bearing (which will appear later), in order to make clear the nature of the problem and the precautions which had to be taken in the investigation to avoid confusion between the various distinct processes.

On the basis of this division two problems appeared which it was the object of this study to investigate. These were: (1) What is the largest number that can be counted by a single act of apprehension—on the one hand, without expenditure of extra time in taking in each additional object; on the other, without the assistance of association or inference? This is the problem of the limit of perceptive counting. (2) What is the part played by association and inference in our habitual acts of counting?

A third problem might have been added, viz., as to the law by which the time of progressive counting increases with the increase of number—in other words, the rate of progressive counting. This last inquiry was not followed up on account of its great complexity: it would have required a large amount of time to carry out the experiments, and the problem itself presented difficulties, on account of certain disturbing factors entering in, *e. g.*, the eye movements necessary to take in any extensive group of objects. As between the other two problems, the present investigation was more particularly concerned with the first.

[1] To avoid confusion the number-names will be printed with a capital.

II. HISTORICAL.

I may point out, first of all, the close relation that exists between this problem and that of the so-called *area of consciousness*. The area of consciousness (Umfang des Bewusstseins), as understood by the Leipzig investigators, is the sum total of impressions that can be held in consciousness at one time. The classic experiments of Dietze[1] on this topic aimed to determine this sum for a single case (the simplest) by means of groups of successive sounds. The subject was forbidden to count the sounds—he was to determine the difference between two groups after both had been given, by the mere fact of retaining all the members of each group in consciousness at once. The groups were compared as equal, greater or smaller, the hypothesis being that as long as this could be done correctly the subject must have had a simultaneous impression of each entire group. Dietze's subjects were able to distinguish differences correctly up to Sixteen when the sounds were uniform, and up to as many as Forty when each group was divided into subgroups of Eight by rhythmic accentuation. The highest numbers in each case were reached only when the rate of succession of the sounds was most favorable; thus these numbers, if the hypothesis be correct, represent the very maximum area of consciousness. The area of consciousness in the case of counting is a somewhat different thing. In Dietze's problem no mental act was involved *during the experiment* but the retention of the sounds in consciousness as distinct; in counting an active effort is required to bring the units together under the form of a number-concept. Still, I am of the opinion that the two problems really belong to the same category, the difference consisting chiefly in the presence of an act of apperception in counting, while Dietze's experiments involved merely perception. My subjects were not able to *gather in* at once numbers nearly so large as Dietze's could *hold together;* this was to be expected to some extent; but the wide difference between the two results, which will appear later, leads me to question whether Dietze's subjects succeeded altogether in avoiding counting (*i. e.*, progressive counting), and still more whether they

[1] Philos. Stud., 1885, II., 362 ff.

did not rely somewhat upon the length of time, and *infer* the size of the group from this—a tendency which (in another form) I found exceedingly difficult to prevent among my own subjects. In view of the importance and fundamental character of this problem, it seems strange that no one has ever undertaken to repeat Dietze's experiments.

Another problem somewhat analogous to the present one is the number of objects, letters, etc., that can be recognized at the same time. An investigation of this subject was made by Cattell[1] at Leipzig, in connection with his reaction time experiments, by a method of combined simultaneous and successive exposure. The objects were passed across a slit in a screen, the slit being varied in size so that any desired number of the objects could be seen simultaneously. He found that three, four or five letters could be recognized when passing at once—the maximum differing within these limits for different subjects; this was apart from the grouping of the letters into words, which, of course, involves association and is a very different process from the one under investigation.

The problem of counting was taken up by Cattell in a later investigation,[2] where he places it under the head of area of consciousness. Cattell's experiments consisted in exposing to view simultaneously and for a very short period (10σ) a number of lines drawn on cardboard; the subject was required to determine the number of lines on the card; the apparatus employed was a falling screen. In these experiments the method of right and wrong cases was used. The largest number for which the right answers exceeded the wrong varied between Five and Eleven, according to the subject. The higher numbers, however, were only correctly counted by those who had made many trials; this leads to the suggestion that the subject may have become familiar with the number of lines on each card in the course of his practice, and that he may have afterwards judged the number from the width of space occupied by the lines on the card—an inferential process again. On this account Cattell's results seem open to question,

[1] Philos. Stud., 1885, II., 635 ff.
[2] Philos. Stud., 1886, III., 121 ff.

and it was important that they be repeated with such changes in method as would avoid this possible criticism. This was one object in the present investigation.

In connection with these experiments Cattell investigated the number of figures, letters and words, *recognizable* after a very brief exposure. The same apparatus was used. The results are as follows : Figures, 3 to 6; letters, 2 to 5; words, 1 to 4 ; the subjects almost without exception recognized one figure more than they could letters, and one letter more than they could words. This agrees with his previous results, noticed above, by another method. The problem, however, is different from that of counting, and I need not stop to discuss the results in detail.

Numerous other investigations have been made on the recognition time of colors, words, etc., which have only an indirect bearing on the present problem and need not be mentioned here.

III. PRELIMINARY EXPERIMENTS; HAND REACTIONS.

The problem of counting may be investigated, as we have seen, by the method of right and wrong cases; given a short exposure (10σ) of a group of things, how large a group can be apprehended in that time so that the number is known? This treatment of the problem can only be applied to simultaneous, or perceptive counting. It can give no help in the discussion of successive, or progressive counting, and but little in the investigation of inferential counting. A more effective method is that of reaction time. The subject reacts on the number, and the reaction times of the different numbers are compared. This avoids, for one thing, the possibility of counting from the after image. The exposure need not be so short—it should be long enough to ensure the taking in of every member of the group, and is only shortened at all in order to stimulate attention to immediate activity. In the present study the reaction method was adopted as principal; but the method of right and wrong cases served as check upon the results. The times were thrown out whenever the count was wrong; and further, if the wrong answers for a certain num-

ber equalled the right, the determination was set down as a guess rather than a count, unless the right reactions were perceptibly longer than the wrong.

Two separate investigations were made by the writer, both upon visual stimuli, but with somewhat different apparatus. The first series, carried out during the winter of 1895–6, developed a number of practical defects, which were remedied in the second series, made in the winter of 1896–7.

In the earlier series, the apparatus consisted of a large screen, with a slit 6 cm. wide and 16 cm. high, behind which swung a pendulum with a small screen attached; when the pendulum was up (and held in place by an electro-magnet) the small screen covered the slit in the larger one. The slit was on a level with the eyes of the subject, who was seated at a distance of 3 m. Behind the slit and the pendulum was fixed a holder, in which were placed, one at a time, the cards used in the experiment; this holder was of course concealed from view by the small screen when the pendulum was raised. The objects to be counted consisted of small white squares, of 5 mm.; these were pasted in a vertical line at distances of 5 mm. on the cards, which were black. In some cases the distances of the spots and their size were varied. The experimenter sat near the apparatus and was concealed (as well as the chronoscope) from the subject by another screen; he released the pendulum by means of a key. A contact was made at the point where the white spots first became visible to the subject, and the latter thereupon reacted on the number with a Morse key, at the same time calling out the number. The exposure was not limited, the pendulum being held back by a catch so that the spots remained full in view until after the reaction. By watching the (Hipp) chronoscope hands, the experimenter could tell whether the reaction preceded the speech; anticipatory reactions on the mere light stimulus were thus prevented.

Four subjects took part in these experiments, from only two of whom, however (C and G), were any large series obtained. A third (H) was unable to avoid anticipations; many of his results had to be discarded on this account, and he finally abandoned the work. The writer, who was the fourth subject (W),

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ber equalled the right, the determination was set down as a guess rather than a count, unless the right reactions were perceptibly longer than the wrong.

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Four subjects took part in these experiments, from only two of whom, however (C and G), were any large series obtained. A third (H) was unable to avoid anticipations; many of his results had to be discarded on this account, and he finally abandoned the work. The writer, who was the fourth subject (W),

acted as experimenter most of the time, in order that the other subjects might not become too familiar with the appearance of the cards. The experiments were conducted in the daylight. On ordinarily bright days the spots were easily distinguishable by the subjects, and were yet close enough together to come within the range of clear vision, so that no eye-movements were necessary to distinguish them.

The method was open to the following criticisms: (1) On cloudy days the spots were less easily discernible than on bright days; it was impossible to measure the illumination or determine the effect of its variations upon the reaction time. (2) There was found to be a tendency on the part of the subjects, after a certain amount of practice, to *judge* the number of spots by the amount of space they covered on the card, *i. e.*, the length of the broken white line which they formed. (3) While it was possible for the experimenter to distinguish anticipatory *light* reactions, in the manner above mentioned, slight anticipations could not be detected; furthermore, (4), the attention being divided between the hand and the voice, the reactions themselves might not be reliably uniform. While this last objection did not appear to the writer to be borne out by the actual results, it was obviated in the second series by the use of a mouth key for the reactions; the third objection was met by this same change. The second objection was partly met in the earlier experiments by varying the size of the spots and their distance apart; but the conditions of the apparatus prevented this from being available—or at least effective—for numbers greater than Five; with larger numbers there was no room in the slit for greater distances, and with distances less than the normal the spots were difficult to distinguish; if larger or smaller spots were used, the new cards soon came to be recognized and judged as well as the original. In spite of the defects of this method, the results obtained are of service to compare with the later ones. They are also of value in themselves in several particulars.

There were in all 40 sittings in this series, of which 19 were made by C and 9 by G; in each case two sittings were set apart for preliminary practice in simple reaction; the

results of these are not included in the tables. At the beginning of each regular sitting, before the counting reactions were begun, a series of 10 sensory reactions was taken on a card with four spots; a motor series was sometimes taken also. The subject C was of a distinctly sensory type, as these results show (Table I.) and as was proved by repeated tests elsewhere.

TABLE I.—SIMPLE REACTIONS; VISUAL, HAND, IN LIGHT.

	S	MV	No.	SER.	M'R	MV	No.	SER.
C	291.9	52.9	94	10	324.3	58.4	91	9
G	351.9	74.4	60	6	285.9	69.7	39	4
H	244.3	33.	40	4	222.2	28.3	30	3
W	235.6	58.9	43	4	179.3	40.4	21	2
C st.	192.1	34.8	50	3	247.8	58.6	30	2
C st. at.	185.9	26.9	10	1	——	——	—	—

S = sensory; M'r = motor; MV = mean variation; No. = number of reactions; Ser. = series of reactions; st. = reaction on strip of white paper; at. = reaction with great attention. The times are given in σ = .001 sec.

G and W were of the ordinary motor type; H was slightly motor. In Table I., C's first few series are omitted, as it was found that he frequently anticipated on account of a slight sound made by the pendulum in starting; this defect was remedied in all the later sittings. To determine the relation between these results and ordinary light reactions, four series were taken with a long white strip as stimulus in place of the spots; the results are given in the last two lines of the table; in one of these series (st. at.) the subject concentrated his attention to the utmost.

The reaction times on numbers are given in Table II., the sensory time for each subject being given first for the sake of comparison. The counting time for One is seen to be in every case over 100σ longer than the sensory time. As regards the relation between the times for the different numbers, I will delay comment until the later experiments have been presented.

TABLE II.

COUNTING REACTIONS; HAND, IN LIGHT.

	C			G			H			W		
	M	MV	No.	M	MV	No.	M	MV	No.	M	MV	No.
S	291.9	52.9	94	351.9	74.4	60	244.3	33.0	40	235.6	58.9	43
I	407.4	56.1	70	523.6	96.4	30	429.4	62.2	19	497.2	112.0	9
II	415.4	60.5	77	532.9	85.6	38	419.7	74.7	30	416.3	(50.0)	6
III	481.4	77.9	89	575.6	118.0	33	466.1	77.1	23	514.7	106.6	9
IV	620.8	94.4	74	652.7	108.5	41	600.2	118.9	16	613.8	(157.0)	5
V	934.8	195.9	53	853.5	231.6	35	828.8	(426.2)	5	638.8	(138.8)	8
VI	1274.7	311.9	47	1127.9	364.2	20	1167.7	(54.7)	4	(841.3)	(402.6)	3
VII	1783.0	448.5	26	1892.4	(206.1)	8	—	—	—	(1635.0)	—	1
VIII	1901.0	(172.0)	5	2369.2	(278.7)	4	—	—	—	(1782.5)	—	2

Roman numerals signify number of spots reacted on; S = sensory reaction; M = mean reaction time.

(Table III.) Table III. shows the number and percentage of errors to the entire number of reactions; in no case (except with H) was the number of errors so great as to suggest that any other process but actual counting was used.

TABLE III.—ERRORS IN COUNTING; LONG EXPOSURE.

	C(19)			G(9)			H(6)			W(6)	
	No.	E	%E	No.	E	%E	No.	E	%E	No.	E
I	70	0	00.	30	0	00.	19	0	00.	9	0
II	77	0	00.	38	0	00.	30	0	00.	6	0
III	94	5	05.3	33	0	00.	23	0	00.	9	0
IV	81	7	08.6	41	0	00.	22	6	27.2	7	2
V	65	12	18.8	36	1	02.8	7	2	28.6	9	1
VI	49	2	04.1	22	2	09.	7	3	42.8	3	0
VII	29	3	10.3	11	3	27.2	—	—	——	1	0
VIII	7	2	28.6	4	0	00.	—	—	——	2	0

The numbers in brackets represent the series taken.

IV. EXPERIMENTS IN COUNTING, WITH MOUTH REACTION.

In the second series artificial illumination was used. A lamp giving practically uniform light was placed in a large enclosed space, within which the pendulum swung; the room was darkened. In the front side of the enclosure was an opening 12 cm. square, but a pyramidal tube extending out 35 cm. reduced the aperture through which the light could pass to 6 cm. square, and prevented its diffusion. Attached to the pendulum was a screen large enough to cover the aperture throughout the entire pendulum-swing; in this screen was a slit 25 mm. wide. The card holder was placed in front of the opening at a distance of 1.5 m., and was illuminated during 131σ when the pendulum swung; as the pendulum was held on the farther side by a catch there was but one illumination of the card before each reaction. The subject sat near the enclosure, and at a distance of 2 m. from the card; the latter was turned at such an angle (ca. 10°) as to prevent any sheen disturbance.

The cards used in these experiments were 16.5 cm. square; the spots were (in every case) circles of 14 mm. diameter, and were placed (in the main series) at uniform distances along the circumference of an imaginary circle, so that the center of every spot was exactly 6 cm. from the center of the card. As the spots were not in line, and the distances between them varied in different cards, and as each card could be used in four different positions, the tendency to use any 'inferential' aid in determining the number was believed to be avoided; the results and the testimony of the subjects themselves confirmed this. The spots were 18, 22 and 26 mm. apart, from edge to edge, in different cards; the same card was rarely used twice in succession, and every card was turned a quarter or half way around before using again; the end spots in the row were never on the vertical or horizontal diameters of the circle; these precautions effectually prevented inferential counting. To enable the subject to fixate the card before the experiment, a very dim gas flame was usually placed near and behind it; with one subject the slight illumination of the room was sufficient to show the outline of the card, without giving any indications as to the spots. The Hipp chronoscope was used in these experiments also, but was placed in another room, thus avoiding possible distraction from the sound. The writer, who generally attended to the cards and the pendulum, gave a preliminary signal, by shouting: 'Ready;' the subject then fixed his eyes on the card, and the Hipp was started by the person in charge. The subject reacted by means of a mouth key;[1] in the counting reactions he simply spoke the name of the number into the funnel of the key. There was thus no danger of anticipation, and no division of the attention, such as occurred when the hand key was used.

At the beginning of every sitting a series of from 10 to 20 sensory reactions was taken; the remainder of the hour was occupied with the counting reactions. The principal subjects were two in number, of whom one, C, had taken part in the

[1] The mouth key used in these experiments will be described and figured in a study by Professor Baldwin, entitled 'Type Variations in Reaction Times,' which will shortly appear in this REVIEW.

former series and in many other reaction experiments. The other, T, had never before reacted on visual stimuli. The writer acted as subject in a number of sittings, and his results are included in the tables also; some reactions were made by a fourth subject, Ta, who was called away, however, before the experiments had advanced far; his results are not included.[1]

There were 40 sittings in all, of which 18 were given by T and 12 by C; 10 of each included counting reactions with the mouth key. In the first six sittings with T, the hand key was used; the counting reactions made in this way are not included in the tables, but the sensory reactions are given in Table IV.,

TABLE IV.—COMPARISON OF MOUTH AND HAND REACTIONS.

		S	MV	No.	SER.	M'R	MV	No.	SER.
C	m. d.	298.3	44.7	130	10	476.8	48.7	20	1
	h. l.	291.9	52.9	94	10	324.3	58.4	91	9
	st. h. l.	192.1	34.8	50	3	247.8	58.6	30	2
W	m. d.	378.	55.4	81	5	288.1	22.8	30	2
	h. l.	235.6	58.9	43	4	179.3	40.4	21	2
T	m. d.	362.8	48.3	153	10	343.1	29.7	18	1
	h. d.	260.	23.2	75	4	250.2	31.4	59	3

All are simple visual reactions; m = mouth, h = hand reaction; d = in dark; l = in light; st = reaction on bright stimulus; cf. Table I.

for the sake of comparison; the simple reactions of C and W in the earlier series are also set down in this table along with their speech-key reactions. Of the reactions given in Table IV., only T's included mouth and hand reactions under uniform conditions of illumination; here the difference is close to 100σ, for both sensory and motor, in favor of the hand. In the cases of C and W, the hand reactions (as was observed above) include series in which the light stimulus differed greatly;

[1] The writer wishes to express his thanks to all who took part in the experiments; as well as to Professor Baldwin, for many valuable suggestions on both the practical and the theoretical sides of this investigation.

hence the wide variation in the results—for C a difference of 152.5σ in the motor and of only 6.4σ in the sensory.

In Table V. the simple reactions with the mouth key are brought together; as before, the first two (practice) series of

TABLE V.—SIMPLE REACTIONS; VISUAL, MOUTH, IN DARK.

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T	362.8	48.3	153	10	343.1	29.7	18	1
C	298.3	44.7	130	10	476.8	48.7	20	1
W	378.0	55.4	81	5	288.1	22.8	30	2

Symbols same as in Table I.

each subject are omitted. T appears to be of a slightly motor type, while the earlier results with C and W are confirmed—they belong to distinctly sensory and motor types, respectively.

The two following tables give the counting reactions. In Table VI. the mean reaction time (M) and mean variation

TABLE VI.—COUNTING REACTIONS; MOUTH IN DARK.

	T			C			W		
	M	MV	No.	M	MV	No.	M	MV	No.
S	362.8	48.3	153	298.3	44.7	130	37.8	55.4	81
I	567.1	83.8	23	553.1	62.6	19	573.3	75.	6
II	621.1	93.8	21	545.5	45.9	23	597.	(132.0)	3
III	655.	86.8	18	683.7	125.4	19	572.5	68.7	9
IV	683.8	123.1	42	740.4	91.2	35	588.8	(107.4)	5
V	812.3	155.8	34	1090.1	316.3	27	655.7	141.7	8
VI	938.1	154.6	17	1411.2	313.2	17	675.	53.6	6
VII	1265.	(26)	3	1352.3	362.3	6	786.8	(168.2)	5
VIII	——	——	0	(2828)	——	1	(689.3)	——	3
Zero	939.	(292)	3	831.5	(136.5)	4	717.3	——	3
Infinity	1007.7	434.	16	1128.	——	1	671.7	——	3

Symbols as in Table II.; Zero = reaction on blank card; Infinity = reaction on number too great to count.

(MV) are given for numbers from One up to Seven. The number of (successful) reactions for each number (No.) appears in a separate column. For the sake of comparison the corresponding data of the sensory reactions (S) are added also. As the mean variation is considerable, the reactions can be better compared by means of the upper and lower limits of their variation; these are given in Table VII., together with the

TABLE VII.—LIMITS OF MEAN VARIATION.

	T		C		W	
	Lower.	Upper.	Lower.	Upper.	Lower.	Upper.
S	314.5	411.1	253.6	343.	322.6	433.4
I	483.3	650.9	490.5	615.7	498.3	648.3
II	527.3	714.9	499.6	591.4	465.	729.
III	568.2	741.8	558.3	809.1	503.8	641.2
IV	560.7	806.9	649.2	831.6	481.4	696.2
V	656.5	968.1	773.8	1406.4	514.	797.4
VI	783.5	1092.7	1098.	1724.4	621.4	728.6
VII	1239.	1291.	990.	1714.6	618.2	955.

limits of the sensory reactions. From this table it appears that the counting reactions, even for One and Two, are very much longer than the simple reactions, while the difference between the times for successive numbers in every case (except T for Seven) falls within the limits of mean variation of the next. These results may be expressed under the two following propositions: (1) The shortest counting times are longer than the shortest sensory reactions by about 200σ; and (2) For successive numbers the counting time is approximately the same. Several remarks should be made on each of these statements.

As to the first: the question of the relation between counting and recognition times comes up at once. All the published experiments on recognition time having been made with the hand key, which gives decidedly shorter times than the mouth key here used (cf. Table IV.), it is impossible to compare them

directly with these results. They do admit of comparison, however, with our earlier series. Comparing the latter with Titchener's results reported in the Philosophische Studien[1], we find the following: Titchener gives the sensory time on light stimulus, for three subjects, as 260, 266 and 279σ; and the reaction time on the recognition of a word as 319.3, 317 and 302.8σ for the same subjects. My hand reaction experiments give the sensory times of C, G, H and W, respectively, as 291.9, 351.9, 244.3 and 235.6σ (cf., Table II.); and their counting reactions on One as 407.4, 523.6, 429.4 and 497.2σ. The counting time is thus seen to be somewhat longer than the recognition time, if different subjects can be compared; as it happens, the writer (W) was the first-named subject in Titchener's experiments, which furnishes one case of direct comparison.

Returning to our second proposition, the following interpretation may be given: taking the mean time of counting One for standard, the subject is usually able to count Two, often Three, and occasionally Four and Five in the same time, *i. e.*, by the same kind of simple mental act. With Four or more this seems to be due to a special effort of the attention, or (occasionally) to an expectation of that particular number; in the earlier series there was some assistance from the judgment (inference), but this was carefully guarded against in the present series. In general, then, *it seems to require a longer time, and hence a more complex mental act, to count numbers greater than Three.* For Six and Seven the difference is so marked as nearly to double the length of the reaction time. With these higher numbers, too, other elements come in, as will be seen when we examine Tables VIII. and X., so that the results represent something very different from simple perceptive counting.

In Table VIII. are shown the errors (E) committed in counting each number, and the percentage of errors (% E) to total reactions.[2] In the two last columns for each subject the errors are clas-

[1] VIII., 138–144.

[2] A few reactions are included in this table, from which, through the fault of the apparatus or its operators, no reaction times were obtained, but which are available for the present purpose; this will explain the discrepancy between the figures given in Tables VI. and VIII.

TABLE VIII.—ERRORS IN COUNTING. EXPOSURE OF 131σ.

	T					C					W				
	No.	E	%E	+(∞)	—	No	E	%E	+(∞)	—	No.	E	%E	+(∞)	—
I	31	0	00.	—	—	30	0	00.	—	—	8	0	00	—	—
II	34	0	00.	—	—	30	1	03.4	1	0	8	0	00	—	—
III	35	2	05.7	1	1	30	5	16.7	5	0	12	1	08.3	0	1
IV	58	7	12.1	6	1	50	4	08.	4	0	9	0	00	0	0
V	53	12	22.6	5(1)	6	50	17	34.	8	9	19	4	21.	1(1)	2
VI	51	33	64.7	17(9)	7	50	30	60.	10(1)	19	16	6	37.5	0	6
VII	21	15	71.4	3(11)	1	10	4	40.	2	2	14	4	28.5	3	1
VIII	10	10	100	1(9)	0	5	4	80.	0	4	24	18	75.	(6)	12

sified according as the answers given were too great (+) or too small (—); in some cases, it will be noticed, the subject reacted on discovering that the number was too great to count (∞); these are given in brackets in the *plus* column. It will be seen that the percentage of errors increased steadily (with slight exceptions) in the two principal subjects from Three upwards, until at Eight it reached practically 100. This explains why no reaction times are given for Eight in Table VI.[1] No cards with more than eight spots were used, owing to this fact, but the subjects did not know of this till near the end of the series; it will be noticed that T gave one Nine-reaction and nine 'Infinity'-reactions on Eight.

The conclusion to be drawn at once from a comparison of these tables is that the upper limit of counting without inference and without eye-movements is Seven or less. In T's case the number of wrong answers begins to exceed the right at Six: In C's case it exceeds it at Six but not at Seven (where only ten trials were made). Moreover, if we take into account the 'doubtful cases,' 'guesses' and 'judgments or inferences' (cf. Table X.), *the limit for progressive counting without eye-movement falls in both cases to Five.*

The two subjects differed somewhat in their method of procedure, as shown by the differences in the number of errors,

[1] The bracketed numbers given there were of 'doubtful' reactions; cf. Table X.

guesses, inferences and 'Infinity'-reactions,[1] but their results agree substantially in the limits for the various kinds of counting. Although these results cannot be generalized without corroboration from other subjects, they are of great value as coming from subjects of two distinct mental types, the sensory and the motor. The distinctions which the subjects made between the different counting processes, simple perception of number, inference or judgment, guessing and progressive counting agreed substantially also; these distinctions will be explained and discussed later, in connection with the final series of experiments on inferential counting. The reactions of W are too few in number to be of much service; they present a substantial agreement with the others for the lower numbers; for the higher numbers the times are shorter and the proportion of errors far smaller; this is probably due to the writer being familiar with the individual cards from having made them and handled them in most of the experiments.

Before leaving the present question we may compare briefly the results of the mouth-key experiments with the hand-key experiments of the former series. It appears that the hand reactions are generally shorter; in the case of C, who acted as subject in both series, the difference is very uniform except for the higher numbers. In the hand reactions, it will be remembered, the number remained in view until after the subject had reacted; there was thus an opportunity for 'progressive counting,' which was taken advantage of; so that instead of guessing or inferring the number (as was sometimes necessary in the mouth reactions), the subject would take more time and 'count up' the spots. The smaller percentage of errors and the longer time required to count larger numbers, are indications of this tendency.

V. EXPERIMENTS ON INFERENTIAL COUNTING.

When the main series with the mouth key were practically completed, the subjects T and C were tested with a set of geometrical figures; for example, three spots in the form of a triangle, four in the form of a square, five in a quincunx, etc.

[1] See Table X.; cf. also Table IX., especially the results for Eight.

Of the forms used, some were regular and others irregular. The apparatus and general procedure were the same as in the main series. As the figures had to be frequently changed to avoid mere recognition reaction, there were a great many different ones used, and it is impracticable to tabulate them all. A number of typical examples are given, however, in Table IX., showing the effects of various arrangements. The num-

TABLE IX.—COUNTING BY INFERENCE.

	T				C			
FIGURES.	N	M	N′	E	N	M	N′	E
III	2	784.	2	0	3	595.3	4	0
V	4	836.2	8	0	4	810.	4	0
VI	6	1181.8	8	1	6	1546.6	7	1
VII	8	1051.1	9	0	9	1452.2	9	0
VIII	0	——	4	4	3	3234.3	3	0
IX	2	1108.5	2	0	3	1603.	3	0
XII	2	1631.5	2	0	1	2108.	1	0

ber of successful reactions and mean reaction times are given in the columns headed N and M. The columns headed N′ and E represent the whole number of attempted reactions and errors, respectively, as in Table VIII. It was found that for the higher numbers a regular arrangement facilitated the count, especially where the figure was compact; in the case of a straight line and a polygon of six sides or more the regularity rather impeded it; the count was still more impeded where the arrangement was irregular.

The fact that numbers as high as Twelve were correctly counted after so short an exposure shows at once that the process employed was different from that employed in the regular experiments. This is confirmed by the after-judgments of the subjects, who described the procedure as 'inference,' 'counting,' and 'guessing.' When these terms were explained they were found to indicate radically different processes. *Inference* was the term used when the number was judged from the shape,

etc., or inferred from the memory of the same figure as seen before. *Counting* was applied to the progressive or 'one, two, three,' counting. *Guessing* was a combination of progressive counting for part of a group, with a guess or judgment of the remainder; it is really a species of inferential counting. The counting of some numbers, such as Nine and Twelve in the table, was performed by a kind of multiplication; the subject called this process inference (or judgment), explaining at the end of the test that he included multiplication under this head.

TABLE X.—CHARACTER OF REACTION AND COUNT.

		II	III	IV	V	VI	VII	VIII
T	Whole number of reactions	34	36	59	53	55	27	12
	Errors	0	2	7	11	24	4	1
	Reactions on light	0	1	1	0	2	2	0
	Too large; no reaction	0	0	0	0	2	4	2
	Too large; reaction (∞)	0	0	0	1	9	11	9
	Inferences	0	0	0	0	3	1	0
	Guesses	0	0	0	6	8	2	0
	Counts	0	0	0	0	3	1	0
	Unspecified	34	33	51	35	4	2	0
	Doubtful	0	1	1	4	1	3	0
C	Whole number of reactions	30	30	50	50	51	10	5
	Errors	1	5	4	17	29	4	4
	Reactions on light	0	0	0	0	1	0	0
	Too large; no reaction	0	0	0	0	0	0	0
	Too large reaction (∞)	0	0	0	0	1	0	0
	Inferences	0	0	0	1	2	0	1
	Guesses	0	0	2	8	4	3	0
	Counts	0	0	0	4	8	0	0
	Unspecified	29	25	44	20	6	3	0
	Doubtful	0	0	1	3	7	2	1
W	Whole number of reactions	8	12	10	20	16	14	25
	Errors	0	1	0	3	6	4	12
	Reactions on light	0	0	1	1	0	0	0
	Too large; no reaction	0	0	0	0	0	0	1
	Too large; reaction (∞)	0	0	0	1	0	0	6
	Inferences	0	0	0	0	0	1	0
	Guesses	0	0	0	0	0	1	0
	Counts	0	0	0	0	0	0	0
	Unspecified	8	11	9	15	10	8	6
	Doubtful	0	0	1	2	4	1	4

In Table X. are shown the processes used in the main series of counting experiments (cf. Tables VI. and VIII.), as described after each reaction by the subject; inferences and guesses were always reported; a large proportion of the successful reactions on numbers higher than Four, which are not expressly ascribed to one or other of these processes (those in the row labeled 'unspecified'), are undoubtedly cases of progressive counting ('counts').[1] No attempt was made in the present study to distinguish between inference and association; in the table both processes are included under the term inference. The word judgment, which was sometimes used by the subjects in place of inference, has been generally avoided in the discussion as being too broad and indefinite. Since 'guessing,' as here used, is a complex process whose chief characteristic is an inference, this leaves but three distinct processes by which the subjects gained their knowledge of the numbers, according to their own statements, viz., the processes which we have called perceptive, progressive and inferential counting.

VI. CONCLUSIONS.

Referring back to the questions proposed at the outset, we find that definite, if not complete, answers can be given to both as a result of the present investigation.

1. *The Limit of Perceptive Counting*—The limit of perceptive counting, with two adult subjects (T and C) one of motor and one of sensory type, both intellectually bright, but with no special talent for numbers, was found to lie at Four; this number was occasionally grasped and reacted on in the same time as One, but only of Three and Two could this be said generally. Investigations of other subjects (G, H and W, as well as C again) with hand reactions served to confirm this view. We conclude, therefore, that, *except under special stress of attention, or with subjects especially apt in this direction, the function of perceptive counting is limited to the numbers One, Two and Three.*

[1] In Table X. the rows are mutually exclusive, except the first and last; the 'whole number of reactions' equals the sum of the other rows, leaving out the row of 'doubtful' answers.

2. *The Rôle of Inference.*—To apprehend numbers greater than Four, then, some other function must come into play. The process by which this knowledge is first attained is what we have termed 'progressive counting.' It consists in establishing what mathematicians call a 'one-to-one' relation between the objects in the given group and the series of natural numbers; when the group is exhausted the last number reached in the count is known to be the number of objects in the group. But this process is comparatively slow, and in practice it is often shortened by one or another device. Thus we know by frequent experience (*e. g.*, with playing-cards or dominos) that the figure called a quincunx is a group of five things; when, therefore, we see such a figure, instead of counting the spots progressively, we *associate the number-name* (Five) with the group; and so of other figures which have become known by repeated experience. Or, again: given three rows of three spots each, although this particular figure may not be familiar to us, still we know from the multiplication table (which *is* familiar enough) that 'three times three is nine,' and upon perceiving the three spots on each side we immediately associate the number Nine with the group. A third case, not touched on in any of our experiments, is where the group is divided into sub-groups of various sizes; here we may count the sub-groups separately—by the perceptive or progressive processes—and reach the sum total at once through our knowledge of the addition table; this is another instance of inference based on association. Now it appears from our final series of experiments (Table IX.) that for the higher numbers the use of this inferential process shortens the reaction time, but that for the lower ones it does not—in fact, it tends rather to lengthen it. In other words, *inference tends to shorten progressive counting and to lengthen perceptive counting*, when it takes their place wholly or in part.

It would be useless to attempt to measure the *amount* of shortening produced by influence, since the time of the latter process itself varies within wide limits. In cases where we are very familiar with a certain grouping it may reduce the time enormously; in others, the inferential process is so complex that it is of little use in expediting the count. The chief result

of the present experiments, as regards the higher numbers, is to show that progressive counting is a comparatively long process, and that we must resort habitually to some kind of inference in counting large groups. Familiar figures are rare, and are practically confined to groups of less than a dozen; but addition and multiplication, combined with perceptive or progressive counting are common resources. As a matter of fact, we do not often have occasion to count very large groups; when we do, we usually fall back upon one or other of these inferential processes.

VII. Some Experiments on the Successive Double-Point Threshold.

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Nearly all of the experiments on the tactual double-point threshold have been carried on by the method of least changes, the original of which was first conceived and applied by that father of experimental methods, E. H. Weber. Of the very large number of discussions in this field, which have appeared since the original discussion of Weber, only one[1] investigates the double-point threshold with successive stimuli. At the same time, it has been well known by every experimentor in this field that the threshold for the perception of successive points must be much shorter than that for the perception of simultaneous stimuli on the same spot; for the effect of any slight failure to set the two points upon the skin simultaneously is always the perception of the two points before the ordinary threshold has been reached. The following is the report of some experiments which, if not sufficiently numerous to entitle them to the claim of completeness, may nevertheless be helpful as preparatory to a more protracted study.

The object of the experiments was to determine the thresh-

[1] Judd, Ueber Raumwahrnehmung im Gebiete des Tastsinnes, Phil. Stud., Bd. XII., 409–463.

old for the perception of spatial difference, and that for the direction of the difference (in eight different directions from the same spot of skin) with different intervals of time between the stimulations. Among the problems which were expected to appear in the course of the experiments were the following: (1) is the threshold for the perception of spatial difference, with successive stimuli, the same as that for direction, or is it different? (2) are they the same for all directions from the same spot of skin? (3) do they vary for different lengths of time-interval between the stimuli, and if so, according to what law? (4) is any light to be gained from these results upon simultaneous stimuli, and upon the general question as to the nature of tactual space-perception?

The subjects of these experiments were four, Professor H. C. Warren (W), Mr. J. F. Crawford (C), Dr. C. W. Hodge (H), Dr. G. A. Tawney (T). Excepting the latter none of these had any practice in the performance of such experiments. (T. had taken part in an extended series of experiments on the tactual double-point threshold for simultaneous stimuli.)

The arm of the subject rested upon the table, a screen concealing it and the apparatus from him. A piece of wood was so placed that the subject could grasp it, and thus preserve the same position of the arm during each hour. The spot investigated was also secured by marking the point on the skin which was first touched in each experiment. The temperature of the room was kept approximately constant, and the general conditions of the experiments, such as mental preoccupation, the mood and the health of the subject, the time of day, etc., were carefully noted before each hour.

The instrument used was a Verdin æsthesiometer. In order to facilitate the experiments, the instrument was suspended by a cord which passed over a pulley to a swinging weight. A difficulty arose in the determination of the distance on the arm of the second point stimulated from the first. We wished to use both points of the instrument in order to take advantage of the millimeter scale of the æsthesiometer, but the points could not rest upon the arm at the same time. One of the points was accordingly elevated by inserting a piece of wood

beneath the indicator, the other point remaining extended to the full length of the spring. A card containing a small hole and fastened to a piece of wood was so placed that by passing the points of the æsthesiometer through the hole successively, the same spot on the skin could be touched by the extended point and then pointed to by the elevated point. In this way the distance of the second stimulation from the first could be read from the æsthesiometer scale without touching the arm with both points. The hole in the card was suspended over the same spot on the skin from day to day. The experiments were conducted in the following manner: The extended point was first passed through the hole in the card with a pressure against the skin of about 50 g. The æsthesiometer was then raised and the elevated or shortened point was made to pass through the hole and point to the spot just touched by the other point, while the other point pressed the skin at a distance measured by the horizontal graduated bar of the instrument. This second pressure was also about 50 g. The points were of bone suitably rounded off so as not to cause pain.

To regulate the duration of the stimulations, a metronome was made to vibrate at the desired rate in an instrument case across the room. The duration of the stimulation, as well as that of the interval between the stimulations, was regulated by counting the beats of the metronome. Another precaution was found necessary with reference to the pressure of the points. By the conditions of the experiments the first of each pair of stimulations occurred at the same spot on the arm throughout the series. But the repeated stimulation of this spot gave rise, in some cases, to a qualitative difference between the sensations, which soon came to be recognized by the subject as pain. Thus the second point might be recognized as different from the first without any perception of spatial difference. This, it is true, is an inference wherever it occurs, whereas the answers of the subject ought to be direct perceptions; but he very easily, as experience proved, mistakes his inference in this case for an act of perception, and even though he should not do so, it is very probable that the inferred knowledge that the points are in fact not the same would have a pronounced effect upon his answers.

To avoid this result, we simply lessened the pressure upon the first point as the hour proceeded, asking the reagent to inform us whenever any qualitative or quantitative differences appeared between the two sensations. It has been asserted that one of the conditions of these and similar experiments is that the two sensations be subjectively the same in intensity, and it is usually assumed that this is to be secured by the same objective pressure. But one finds in fact that two points on the skin are very seldom equally sensitive to the same objective pressure. The only adequate method of securing like subjective intensities is the empirical one of testing the two spots until we have ascertained their relative sensibility.

The object of the first series of experiments was to determine the threshold for the perception of spatial difference in two successive stimulations, and also that for the perception of the direction of the second stimulation from the first. The interval between the two stimulations was a constant one of three seconds in this series of experiments. Eight directions were chosen in all, viz. up (toward the shoulder), down (toward the hand), in (toward the little finger side of the arm), out (toward the thumb side of the arm), up-out (half way between up and out), and similarly down-out, down-in, up-in. According to the method of least changes, the series in any one direction should be reversed and the average drawn from the two thresholds thus obtained. As the direction of the second point from the first is known in the reverse series of these experiments, it seemed best to separate the two series and not to follow the usual custom of taking the average between the two. The question also arose whether the thresholds for the diagonals might not be different from those for the axes, owing to the direction, and it was decided to take the thresholds for the four axes first, then proceed to the thresholds for the diagonals, and lastly to take the eight directions together. In the latter case, the eight directions could not be taken without readjusting the apparatus and, rather than do this (which would notify the subject of the direction), one of the directions, viz. up, was omitted. The experiments on H were performed by T, and those on T by H. Tables I. II. and III. show the results of the di-

TABLE I.

Showing thresholds of difference and thresholds of direction on the axes from the originally stimulated point; also middle threshold and middle variation.

HODGE.

IN.		DOWN.		OUT.		UP.		Each Day.	Aver. Thresh.
Difference.	Direction.	Difference.	Direction.	Difference.	Direction.	Difference.	Direction.		
0	6	2	2	0	6	2	4	1.0	4.5
1	14	1	6	2	2	2	2	1.5	6.0
1	9	2	3	0	2	1	8	1.0	5.7
3	3	4	4	1	7	2	6	2.5	5.0
3	20+	1	20+	3	4	3	3	2.5	3.5
2	32	3	5	3	5	5	5	3.3	11.
20+	5	3	7	3	5	4	4	3.3	5.0
4.3	12.7	2.3	6.7	1.7	4.4	2.7	4.5	M. Thresh.	
4.5	8.1	.9	4.1	1.2	1.5	1.1	1.6	M. Var.	

TAWNEY.

2	6	2	4	0	6	2	3	1.5	4.7
1	2	1	3	2	4	1	1	1.2	2.5
1	2	1	2	2	2	3	3	1.7	2.2
1	8	3	3	2	5	2	2	2.0	4.5
0	5	3	5	2	3	1	3	1.5	4.0
2	2	1	3	4	4	3	3	2.5	3.0
1.2	4.1	1.8	3	2	6	2	3.7	M. Thresh.	
	2.1		.6		2		1.2	M. Var.	

WARREN.

1	14	1	12	1	12	20+	20+	1.0	12.5
3	10	2	9	2	11	1	20+	2.0	10.0
5	12	2	8	1	8	2	4	2.5	8.0
3	12	1.6	9.6	1.3	10.3	7.6	14.6	M. Thresh.	
1.3	1.3	0.4	1.5	0.4	1.5	8.2	7.1	M. Var.	

TABLE II.

Showing thresholds of difference and thresholds of direction on the four diagonals from the point originally stimulated, the middle variation and the middle threshold.

HODGE.

DOWN–IN.		DOWN–OUT.		UP–IN.		UP–OUT.		Each Day.	Aver. Thresh.
Difference.	Direction.	Difference.	Direction.	Difference.	Direction.	Difference.	Direction.		
5	6	1	5	1	11	4	4	2.7	6.5
3	6	4	4	2	5	2	3	2.7	4.5
2	7	3	5	3	3	3	3	2.7	4.5
1	5	4	5	2	5	5	5	3.0	5.0
4	5	3	3	2	6	4	4	3.2	4.5
3	5	1	2	4	12	1	1	2.2	5.0
4	6	3	3	3	6	1	6	2.7	4.2
3	5.7	3	3.8	2.5	6.8	3	3.7	M. Thresh.	
.9	.6	.9	1.2	.8	2.3	1.3	1.1	M. Var.	

TAWNEY.

2	6	3	5	2	2	1	3	2	4
3	3	3	3	1	1	5	6	3	3.2
1	2	0	1	2	2	1	1	1	1.5
0	3	2	7	2	2	1	8	1.2	5.0
1	2	2	3	1	2	1	2	1.2	2.2
1	2	2	6	1	5	1	3	1.2	4.0
1	3	1	2	1	2	1	5	1	3.0
1.3	3	2	3.8	1.4	4	1.6	4	M. Thresh.	
.7	.8	.7	1.8	.5	2	1	2	M. Var.	

WARREN.

1	10	1	10	1	6	1	2	1	7
1	7	1	7	2	2	1	1	1.2	4.2
1	8.5	1	8.5	1.5	4	1	1.5	M. Thresh.	
0	1.5	0	1.5	.5	2	0	.5	M. Var.	

TABLE III. *Showing the thresholds of difference and thresholds of direction on different days, in seven different directions from the point originally stimulated, also the middle thresholds and middle variations.*

	IN.		DOWN.		OUT.		IN–UP.		IN–DOWN.		OUT–DOWN.		OUT UP.			
	Difference.	Direction.	Difference.	Direction.	Difference.	Direction.	Difference.	Direction.	Difference.	Direction.	Difference.	Direction.	Difference.	Direction.	Each Day.	Aver. Thresh.
HODGE.	3	14	3	3	2	2	2	11	2	2	2	5	20+	20+	2.3	6.1
	2	14	3	3	3	5	4	8	3	7	3	3	2	12	3.0	7.2
	0	11	3	3	1	8	1	4	5	7	1	5	3	3	2.0	5.4
	4	11	4	4	3	3	3	7	3	6	3	3	3	3	3.3	4.8
	3	20+	2	20+	20+	10	0	20+	4	4	20+	5	20+	20+	2.2	2.7
	2.4	14	3	6.6	5.9	5.6	2	10	3.4	5.2	5.9	4.2	9.6	11.6	M. Thresh.	
	1.1	2.4	.4	5.3	5.7	2.7	1.2	4.4	.9	1.7	5.7	.96	8.3	6.9	M. Var.	
TAWNEY.	0	1	20+	20+	20+	6	20+	1	20+	2	0	1	1	2	1	2.1
	1	4	1	2	1	7	0	1	1	5	1	2	1	8	1	4.1
	1	3	1	1	0	7	0	1	1	2	1	5	1	5	.7	3.4
	2	5	1	4	2	9	1	1	1	3	3	3	3	4	2.	4.1
	2	2	2	2	0	3	2	4	2	2	2	3	1	2	1.6	2.5
	1.2	3	5	5.9	4.6	6.4	4.6	1.6	5	2.8	1.4	2.8	1.4	4.2	M. Thresh.	
	.4	1.2	6	5.7	6.1	1.5	6.1	0.9	6	0.9	.9	1.04	.64	1.8	M. Var.	
WARREN.	2	20+	20+	20+	1	20+	20+	20+	1	13	1	13	1	5	1.5	10.3
	1	3	1	6	1	5	1	1	1	5	2	2	2	2	1.3	3.14
	1	6	1	3	1	3	1	4	1	15	1	3	1	2	1	12.
	1.3	9.6	7.3	9.6	1	9.3	7.3	8.3	1	11	1.3	6	1.3	3	M. Thresh.	
	.4	6.8	8.4	6.8	0	7.1	8.4	7.7	0	4	.4	3.6	.4	1.3	M. Var.	

rect series of experiments on the axis, on the diagonals, and on the axis and diagonals combined, the thresholds for difference and direction being given side by side. The number 20 indicates cases where a wrong suggestion prevailed to such an extent that a correct answer was never reached.

What impresses us first on looking over the tables is the difference between the two thresholds, *i. e.*, between that for spatial difference between the two stimulated points on the arm, and that for the direction of the second point from the first. This difference has lead the writer already referred to to the conclusion that the threshold for the tactual perception of spatial difference is shorter than that for the tactual perception of spatial direction under the conditions of these experiments. But when we consider that the idea of direction is inseparable in thought from the idea of spatial difference, it seems improbable that there should be a perception of spatial difference without a perception of the direction of one point from the other. In other words it seems difficult to sense spatial difference without direction. And yet we are told that this is the real nature of all those cases where the threshold for the perception of difference is shorter than that for the perception of direction. This means that space is after all not the form of tactual perception; at least, that space in three dimensions is not.

But again, the most of the cases, upon which this inference rests, do not pretend to be perceptions of spatial difference without any direction. A direction is usually given by the subject, but it happens to be false, and the inference is drawn by the operator that a difference is perceptible, but not the direction of the difference. Is it not possible that a difference is perceived which is not spatial at all, and that the subject comes to give it the worth of real space-perception by illusion? It should be remembered that we possess an objective standard for determining the direction-threshold, such as we do not possess for the difference-threshold. In the case of difference alone, the answer is usually correct, because a difference is, as a rule, actually present; but in the matter of direction we take the correctness of the answer as a criterion of a real perception of direction. Is it not possible that there may exist an illusion

as to difference as well as to direction? After the discovery which has recently been made of the enormous part played by suggestion in the perception of two simultaneous points,[1] it is at least possible, not to say probable, that the same law works here also. Of this we can speak more advantageously later. What we are concerned here with is, first, that we have no right to apply an objective criterion of true perception in the case of direction unless we can apply the same standard in the case of difference; we should refrain from generalizing until the facts of the case have been more thoroughly looked into; secondly, this generalization, that the difference threshold is the smaller, would in no case be acceptible if it were possible to account for the observed facts by such a well established law as one which has been included under the general term, the association of ideas, but which we prefer to call suggestion. The direction given by the subject may be wrong, as it often is; but this merely constitutes a mistake of perception which, where persistent in any one direction, we call illusion.

Assuming that the apparent perceptions of difference without direction are not bona fide instances of perception in all respects, what explanation can be given for the errors in the judgments of direction? It seems as though a very natural explanation is to be found in experiences with which we are made familiar every day and hour. The perception-act in these experiments differs from that of ordinary experience in the fact that the subject is not allowed to see the spot stimulated and the instrument stimulating, at the same time he feels the touch. That constant practice of testing our tactual sensations by the sensations of a much more highly developed organ is therefore not possible here; and, consequently, the assimilation of the present impressions goes on by means of visual and motor images, as every one can easily persuade himself by trying the experiments on himself. Just as in reading we pass over typographical errors without being in the least conscious of their presence, because the actual visual images are assimilated to a correct visual image of the arrangement of the words and letters; so in these experi-

[1] Tawney, Ueber die Wahrnehmung zweier Punkte, etc., Phil. Stud., Bd. XIII., S. 163 ff.

ments, one assimilates the actual tactual impressions to a revival copy of similar experiences in the past, but to a copy which is actually not in accordance with the facts because the association bond between the tactual stimulus and the visual or motor image is not sufficiently close to be accurate. The local sign of the tactual sensation is, as has been said before, no simple quality of the sensation itself, but just this associational bond between the sensation and the visual or motor image to which it is assimilated. The question of most interest is, what determines the visual or motor image to be of this or that sort. One finds that both difference and direction are sometimes given when the same point has been stimulated twice in succession, that the direction given, even when the points are actually different, is often wrong, and that the error in the direction judgment seems to lie persistently in the direction of the judgments in the last series of experiments, in a direction suggested by the operator himself, or in a direction which the subject gets by autosuggestion. In some cases it is probable that more than one of these causes are present to determine the subject's answer.

Taking up the answers in which an actual difference is present between the two stimulations, but in which the direction given is wrong, they may be divided into three groups. Some follow some external suggestion, *i. e.*, they are influenced either by a previous judgment, or by the combined influence of the previous judgment and the stimulus. An illustration of the former sort would be as follows: Supposing that an up series has just been taken, and that the present series is one in which the distance increases downward, the subject gives the answer 'up.' The following would be an illustration of the latter: Supposing that an in series has just been taken, and that the present series is progressing downward, the subject answers 'down-in.' Out of 11 such wrong answers made by H during these experiments, 3 fell under the first head and 8 under the second. In the case of W, out of 13 wrong answers of this kind 9 belong to the first class and 4 to the second. T made only one wrong answer of this sort, belonging to the first class.

A second group of cases in which wrong answers were given admit of explanation as instances of autosuggestion.

From an objective point of view they seem to be accidental. The subjective process involved seems to be somewhat as follows: The subject feels the first and then the second stimulus, different in time, and at once strives to assimilate the two impressions to his past experiences. He represents the second impression in this or that direction from the first, in order to see whether the actual impression seems any different from the mental image of past experience. The tactual impression being very vague in space quality, he receives no correction, *i. e.*, the image and the impression seem the same and at once fuse into one perception. All perception seems to involve some such process as this. Every presentation is composed partly of elements of the present stimulation and partly of elements of past experience. The present sensation gives to the whole the vivid character which it itself possesses. Illusion always arises whenever the representation elements of the experience dominate over the whole so as to give it a meaning which the actual sensation elements do not possess. Of course this does not explain the cases at hand; it merely suggests a possible way in which the erroneous judgments of direction come to be given below the threshold for the perception of two points.

Another group of answers seem to be due, in one case, to autosuggestion combined with a stimulus element, and, in another case, to autosuggestion combined with the influence of the previous judgment. Of the former sort one finds in the answers of H 25 instances, and of the latter, 1; in the case of T, 37 of the former, and 3 of the latter; in the answers of W, 46 of the former kind and 16 of the latter.

Granting the hypothesis of suggestion to start with, it seems that all of these instances of wrong answers as to direction illustrate one form or another of the same process.

This conclusion seems the more probable when we consider the group of answers in which difference and direction are both given, while the same point on the skin is stimulated twice. This occurs much oftener in descending series than in ascending series, because in the former the actual direction of the second point from the first is distinctly felt in the first experiment of the series, and this knowledge operates as a sug-

gestion after the difference between the points has disappeared. In the similar experiments of Dr. C. H. Judd,[1] the smallest threshold for the perception of spatial *difference* in descending series is 0, as given in his table. All such instances are obviously due to some sort of suggestion. They correspond to the Vexirfehler in experiments with two simultaneous stimuli. In the case of ascending series the suggestion may be automatic, in descending, external, *i. e.*, from a previous judgment. In the ascending series, however, it may also be due, as above, to the influence of a previous series of judgments or of experiments. A test of this hypothesis, which seemed to be crucial, occurred in the often repeated answer of H, 'spatial difference without direction;' but upon reflecting upon the subjective process involved, he believes these judgments to be at bottom inferences, based upon slight qualitative differences in the two stimulations. No direction can be given, simply because they are inferences; were they perceptions they would be perceptions of direction, though erroneous. In the answers of W, two instances of this phenomenon are to be found. When questioned as to the subjective process involved, he replied, in the first instance, that he had not paid close attention to the first stimulus, and felt, when the second came, that it must be different because of the previous answers which he had made in the series, but that he had no idea whether the direction was the same as in previous instances or not. In the second instance he observed that a certain direction was present in his visual image of the point stimulated, but that he simply was not sure as to the correctness of the representation. Such cases did not occur with T.

A modification of this class of cases is seen in answers which indicate partial location, as, *e. g.*, where the answer is 'up or up-in,' 'out, up-out, or up.' Here the uncertainty as to the correctness of the mental representation is limited to a few alternatives. Where this occurs with H, who is a poor visualizer, the answer seems to be the result of self-questioning as to the probable direction in which the series is progressing. In the case of W, who is a good visualizer, it seems to express

[1] Loc. cit., pp. 420, 421.

uncertainty as to the correctness of the visual image, which, as he says, is usually present in these experiments. These cases also never occur with T.

TABLE IV.—SUGGESTIVE PROGRESSIONS.

Showing the number of series in which the influence of suggestion is obvious; the total number of series; the lower and upper limits of thresholds found, together with their mean; the number of single wrong answers; and the ratio of wrong answers which seemed due to suggestion to the total number of wrong answers.

	H.	W.	T.
Number of Progressions	57	21	65
Number of Series	92	21	90
Threshold { Limits	1–32	1–15	1–9
Threshold { Mean	16.5	8	5
Number of wrong answers	442	134	199
Ratio of suggestions to wrong answers	66 :442	87 :134	61 :199

A comprehensive view of the results of these experiments is offered in Table IV. In the upper line the total number of series of experiments in which suggestive influences are apparent, is given for each of the three subjects, H, W and T. Suggestions of different kinds sometimes appear within the same series, as (*e. g.*) when, after an up-series, the subject answers, when the same point is stimulated twice, 'up,' and continues this answer until the second stimulation has reached a distance of 5 mm, in the direction downward from the first; here he answers 'up-in,' and, as the series progresses and the distance becomes greater, 'in,' 'down-in,' and finally 'down.' Here we have the influence of a previous series of experiments and judgments at first dominating, then a combination of the influence of the actual stimulus with that of the previous judgments, and finally the influence of the stimulus alone; illustrating what is called in the table a suggestive progression.

In the second line the total number of series of experiments is given for comparison with the number in which suggestive

influences appear. This line shows, in the case of H, W and T, respectively, that $\frac{57}{92}$, all, and $\frac{18}{18}$ of all the series were influenced in this way.

In the third line the lower and upper limits of threshold-variations, together with the mean of those two, are given to show the result of varying suggestive influences under similar external conditions. It must be remembered, however, that differences of direction are not taken into consideration in this line, and it is true that the threshold for some directions is lower than for others; other factors than suggestion, such as direction, thus come in to vary the threshold, but all other factors combined are not sufficient to account for the wide divergence apparent in these figures.

In the fourth and fifth lines are reported (1) the total number of single wrong answers occurring throughout the experiments, and (2) the ratio of those answers in which suggestive influences are apparent to the total number of wrong answers. A word should be said with reference to the wrong answers which are not referrable to the influence of suggestion. These were mostly the answer 'same,' meaning that the two stimulations seem to be on the same spot. This is the answer which one expects in response to all distances which lie below the threshold. Such answers sometimes occur, however, when the distance is above the threshold, and it is possible that suggestion has played some part in these. We are not in a position to say, however, that it does so, or to what extent it enters in, owing to the absence of objective criteria. Another group of answers were simple expressions of uncertainty and suspense, and are not to be counted among the wrong answers at all. The answer, 'same,' is often given at the beginning of series, *i. e.*, when the two stimulations are really the same, and the answer is then right. The difference between the wrong answers due to suggestion and those in which suggestive influences are not apparent seems to be chiefly this, that in the former class some element of mental content suddenly makes its appearance in consciousness and influences the judgment, while in the latter class nothing arises to modify the usual reaction of the attention to the stimulus; and this difference is what is meant by suggestion.

II.

Another group of experiments was begun in which the series were all descending. In the experiments the direction was necessarily known to the subject, the object of the experiments being to determine where difference and direction cease to be perceived. Table V. shows the results. Wherever no threshold is given, the subject continued to give a difference and a direction at the end of the series when the same point was stimulated twice.

The influence of suggestion is obvious throughout this table in the fact that the thresholds, where they appear at all, are much lower than those of the ascending series. All of the subjects continued to give a difference and a direction when the same point was stimulated twice. In the answers of T, 11 are of this kind; in those of H, 23; and in those of W, 22. These answers are due, we take it, to the same influence which produces the low thresholds.

III.

The following experiments were carried out by Dr. Hodge in the Princeton Laboratory for the purpose of determining what influence, if any, the length of the interval between the two stimulations has upon the threshold for the perception of spatial difference between the two stimulations. But before going on to describe the experiments, we will notice a few facts as to the subjective processes involved, which are closely connected with the foregoing. Professor W. is a good visualizer; he always closed his eyes during the experiments, and gave close attention to the arm and the spots stimulated as they appeared in the visual copy. Both subjects were given to making a judgment of difference or of direction, or of both, at the beginning of series where the same point was stimulated twice in succession. Such errors (corresponding to the Vexirfehler in experiments with simultaneous stimuli) could, in some cases, as above, in the experiments already reported, be accounted for by the influence of the preceding series, or that of a previous judgment, but the two subjects differ very decidedly in this respect. In the case of C such judgments could, as a rule, be traced to such influences,

TABLE V. REVERSED SERIES ON AXES, ON DIAGONALS, AND ON AXES AND DIAGONALS, WITH H, W AND T.

							H							
DOWN.	IN.	UP.	OUT.	D-I.	UP-IN.	UP-OUT.	D-OUT.	IN.	DOWN.	OUT.	UP-O.	UP-IN.	D-IN.	D-OUT.
1	2	2	—	1	1	—	—	1	1	2	1	—	—	1
2	1	2	1	—	—	—	1	1	—	—	—	2	2	—
—	—	1	—											
							W							
2	—	1	1	1	—	—	—	1	—	—	—	1	1	2
—	2	—	—	—	1	1	—	—	1	1	2	—	—	1
							T							
1	1	2	5	—	2	3	1	3	2	2	2	1	1	2
2	2	1	5	1	—	4	3	1	3	3	6	3	2	3
2	1	—	2											

whereas, in the case of W they seemed, as a rule, to follow no law; they seemed to be purely accidental, so far as the outward conditions of the judgments were concerned. This difference corresponds to another which appears in the observations of W, that to him it seemed unnatural to pay attention to sensations of touch alone. He finds that it requires an especial effort to keep the tactual stimulus before the attention, while distraction from slight causes is easy and frequent. W finds it difficult to keep the first stimulus before the attention for the entire period between the first and the second, when this is 15 seconds.

All of these facts would seem to indicate that the perception of two points with W depends chiefly upon the presence of visual associations. It agrees with this that in some series his replies seem at the start to be determined partly by the stimulus and partly by autosuggestion, but as the series progresses it is not the stimulus which triumphs, as we expect, but the suggestion, and no continuance of the series will suffice to correct the persistent operation of the suggestion. Again, in some series, the presence of more than one suggestion is apparent, neither of which seems to be in any way connected with the stimulus. The visual image of the first point stimulated in each of the experiments of a given series seems to grow more and more distinct as the series progresses, showing that the difficulty in judging correctly lies, not in the absence of definite visual representations, but in the absence of the proper association links between the tactual excitations and the corresponding visual images. W seems to rely, as a matter of habit, far more on visual than upon tactual images for his knowledge of the objects with which he comes into contact. The series in which he knew nothing as to the nature of the series, whether it was along the axes or along the diagonals, were much longer than those in which he had some knowledge as to their nature from the start; showing the comparatively weak significance of the stimuli when experienced alone, it requiring much longer to recognize the direction.

With C tactual and motor images predominate; but the association between these images and the sensations is much closer than is the case with W.

C quite frequently remembers the first stimulus by the aid of a motor image of himself stimulating the point with his left hand. One notices in C also a greater tendency to use whatever data he may be able to acquire for *inferring* what the nature of the stimulus may be. He has a habit of assigning first one direction to the second stimulus from the first, and then another until he finds one which brings the series to an end; and no amount of instruction as to how the answers should be made suffices to divert this tendency. He as a rule infers that his answer is not correct whenever the series is continued for more than two or three experiments further. This is for him a constant suggestion, wherever it occurs. It was for W also in some cases, although not so habitually as with C. The latter also gets information as to the direction which probably is being taken in the experiments by remembering the directions which have already been tried. Sometimes he has the suggestion that the direction pursued is one of two or three, and proceeds by a method of elimination to go from one to another until he reaches one which brings the series to an end. Occasionally he forms an hypothesis as to the direction and answers accordingly until the sensations either confirm or contradict it, the process by which he forms the hypothesis being in some cases a purely inferential one and in other cases an associative one. In some cases, as he observes, he has no notion, prior to the stimulus, as to the direction in question, and answers according to the tactual or motor images suggested by the second stimulus. For C the tactual perception for spatial difference is always a result based upon certain qualitative differences between the sensations involved. When asked to describe the qualitative differences referred to, he speaks in terms which to us seem most vague and indefinite, and which characterize nothing, so far as we can determine, which enters into our own experience. To him, however, they have a clear and definite character. It is, moreover, significant that he observes the presence of certain tactual and motor images as an assimilating factor in every perception; how the assimilation takes place he would not undertake to say, although this is probably the same process for tactual and motor images as W describes for visual. No doubt

C's answers were quite often inferences as much as assimilations; but it seems clear that his usual method of answering is one of assimilating his present sensations to tactual or motor images of previous experiences in the perception of two points.

Going on to the experiments conducted by Dr. Hodge to determine the relation between the length of interval between the two stimuli and the threshold for the perception of two points, the following were the results: The interval was determined as before by a metronome which was placed across the room in an instrument case. The intervals chosen were 2 sec., 5 sec., 10 sec., and 15 sec. It was impossible to make the interval shorter than two seconds and preserve the conditions constant, owing to the nature of the apparatus. A longer interval than 15 sec. could not be chosen because of the difficulty involved in retaining the first sensation in memory until the second should follow. The experiments were made in three groups as follows: first, those in which the directions were straight up, down, in, or out (the axes); those in which the diagonal directions were chosen, and those in which the eight directions were all taken within the same hour. One determination was made for each of the four intervals within each hour in order to have the conditions as near the same as possible for experiments which were to be compared. The number 20+ in the following tables indicate the series in which, because of some false suggestion, the subject never succeeded in making correct answers. The o's indicate the series in which, as a result of chance coincidence, the direction hit upon by the subject when, at the beginning of the series, the same point was stimulated twice, happened to be the correct one for that series. In such series it is of course impossible to determine what the real threshold is. The direction thresholds for the perception of two simultaneous points applied in the same regions as the following experiments were as follows: For C, across, 10 mm., up and down, 15 mm., diagonal toward the thumb, 14 mm., and diagonal toward the little finger, 14 mm. For W the same thresholds were: across, 8 mm., up and down, 13 mm., diagonal toward the thumb, 8 mm., and diagonal toward the little finger, 7 mm. None but the direction thresholds are given in the

tables which follow, along with the average thresholds for each day in all the directions. Table IV. gives the experiments with C.

TABLE VI.

Showing the thresholds for the perception of two successive points at intervals of 2 sec., 5 sec., 10 secs., and 15 sec. on left forearm, volar side of C.

2 SECONDS.

AXES.				DIAGONALS.				EIGHT DIRECTIONS.							
in.	up.	d.	out.	u-i.	d-i.	u-o.	d-o.	in.	up.	d.	out.	u-i.	d-i.	u-o.	d-o.
8	6	0	10	3	9	3	7	3	3	0	9	8	2	6	6
4	4	9	1	4	2	7	2	5	1	3	13	3	5	5	5
4	2	2	4	5	5	1	1	5	7	4	7	3	4	3	15
2	4	3	3	1	7	3	0	4	4	3	8	5	6	3	3
4·5	4	4·2	4·5	3·3	5·7	3·5	3·3	4·2	3·7	3·3	9·2	4·7	4·2	4·2	7·2	M.T.

5 SECONDS.

8	3	7	6	4	6	3	5	4	6	0	3	2	8	2	8	
9	3	5	3	5	6	4	1	6	10	3	9	13	10	13	3	
5	1	1	3	2	3	0	1	4	6	8	3	3	3	4	1	
2	1	5	1	3	4	3	4	5	10	3	2	3	6	6	1	
6	2	4·5	3·2	3·5	4·7	3·3	3·6	4·7	8	3·5	4·2	5·2	7	6·2	3·2	M.T.

10 SECONDS.

4	1	5	7	1	4	4	5	7	6	16	1	10	7	11	3	
4	5	11	4	6	3	6	3	10	2	5	3	4	3	1	1	
4	5	1	3	6	3	6	0	4	3	8	15	10	3	9	5	
6	1	3	5	4	3	5	1	5	2	5	6	4	4	13	5	
4·5	3	5	4·7	4·2	3·3	5·2	3	6.5	3·2	8.5	6.2	7	4·2	8.5	3·5	M.T.

15 SECONDS.

0	3	1	2	3	3	2	5	2	1	4	4	0	5	5	2	
6	5	1	1	5	4	5	0	10	6	3	2	8	3	12	2	
1	0	3	2	3	3	7	6	16	5	7	3	7	10	4	3	
4	2	4	1	5	3	5	1	5	0	3	7	4	4	4	4	
3.6	3·3	2.2	1·5	4	3·2	4·5	4	8.2	4	4·2	4	4·7	5·5	6.2	2·7	M.T.

In the column headings d stands for down; u-i for up and in; d-i for down and in; u-o for up and out; d-o for down and out. What one first notices in the table of C's experiments is the difference between the thresholds for successive stimuli 2 seconds apart and the corresponding simultaneous thresholds. For 'across' the latter was 10 mm., while the succession threshold for 'in' is 4.5 mm., and that for 'out' 4.5 mm.; the simultaneous threshold for 'up and down' is 15 mm., while that for 'up' in successive stimuli is 4 mm., and that for 'down' 4.2 mm.; the diagonal simultaneous threshold toward the little finger is 14 mm., while the two corresponding succession thresholds are 5.7 mm. and 3.5 mm.; the simultaneous diagonal toward the thumb also is 14 mm., while the two corresponding succession thresholds are 3.3 mm. and 3.3 mm. In other words, the succession thresholds are much lower than the simultaneous ones nearest corresponding to them. But if we expect that the succession thresholds will shorten in proportion to the inverse length of the interval between the stimuli, we shall find little to confirm the suspicion. The average of all the thresholds for the interval 2 seconds with C is about 4.6 mm.; that for 5 seconds about 4.55 mm.; that for 10 seconds about 5.03 mm., and that for 15 seconds about 4.1 mm., showing a slight decrease of 0.5 mm. between the thresholds for 2 seconds and those for 15 seconds, while that for 10 seconds is considerably longer than that for either 2 seconds or 5 seconds. This is not a sufficiently definite indication to generalize upon.

Going on to Table VII., showing the results of the experiments with W, similar conclusions are to be drawn. That the succession thresholds are shorter by a very appreciable amount (½ to ⅓) than the corresponding simultaneous is obvious. As to the question whether the threshold decreases as the interval increases, however, these experiments unite in indicating an opposite effect of lengthening the time-interval. The difficulty of retaining the first stimulus clearly in mind during the longer intervals, so marked in the case of W, may have had something to do with this result; and, on the other hand, the tendency of C to infer was no doubt assisted by the increase in the length of the interval, as it gave him somewhat more time.

TABLE VII.

Thresholds on the left forearm, volar side, of W., at intervals of 2 sec., 5 sec., 10 sec., and 15 sec.

2 SECONDS.

AXES.				DIAGONALS.				EIGHT DIRECTIONS.							
in.	up.	d.	out.	u-i.	u-o.	d-i.	d-o.	in.	up.	d.	out.	u-i.	u-o.	d-i.	d-o.
20+	12	3	5	6	3	4	1	6	5	13	6	9	0	10	14
17	6	6	0	3	8	3	4	5	0	3	8	1	8	8	2
5	2	2	6	20+	4	3	20+	7	2	0	7	7	3	7	4
3	10	3	3	4	6	7	2	14	5	3	11	3	3	11	10
1.2	7.5	3.5	4.5	8.2	5.2	4.2	6.7	8	3	4.7	8	5	4.5	9	7.5

5 SECONDS.

20+	5	4	11	8	1	7	2	8	2	10	4	4	8	9	5
8	5	5	5	20+	3	5	2	3	12	4	7	6	6	5	7
7	3	8	8	0	7	2	10	10	4	2	20+	5	3	3	5
4	1	2	6	6	5	5	3	4	7	2	11	2	4	0	10
9.7	3.5	4.7	7.5	8.5	4	4.7	4.2	6.2	6.2	4.5	10.5	4.2	5.2	5.6	7

10 SECONDS.

20+	13	0	15	4	3	4	3	15	18	2	8	3	4	5	20+
5	6	2	6	10	6	3	5	18	0	5	7	3	1	2	7
8	6	3	5	8	3	2	4	6	20+	1	9	4	8	4	3
20+	3	4	6	20+	3	2	8	6	4	10	4	9	0	6	6
13.2	7	3	8	10.5	3.7	2.7	5	11	10.5	4.5	7	4.7	4.3	4.2	9

15 SECONDS.

3	9	0	5	6	4	9	5	8	0	7	8	6	8	7	18
5	4	10	13	7	4	9	16	2	20+	7	20+	7	11	10	6
9	8	4	11	10	4	2	8	10	6	6	13	8	3	7	20+
3	3	9	4	7	12	1	3	8	4	5	3	13	6	10	1
5	6	5.7	8.2	7.5	6	5.2	8	7	7.5	6.2	11	8.5	7	8.5	11.2

So far as these two sets of experiments go, therefore, we may conclude that the threshold for successive stimuli is much shorter than that for simultaneous, but that increasing the length of the interval between the successive stimuli does not further shorten it. This may have the contrary effect. Throughout

these experiments it was observed that the same questions as to the relation of the difference to the direction threshold arose, as in the former series of experiments. The answer 'different without direction' was, however, somewhat more frequent in the experiments with successive stimuli, than in those with simultaneous stimuli; a result due, no doubt, to the suggestive effect of the succession.

Table VIII., corresponding to Table IV., offers a summary view of the part played by suggestion in this entire group of experiments. It will not be necessary to add to what has been said concerning the previous table of the same kind.

TABLE VIII. SUGGESTIVE PROGRESSIONS.

Showing the number of series in which the influence of suggestion is obvious; the total number of series; the lower and upper limits of thresholds found, together with their mean; the number of single wrong answers; and the ratio of wrong answers which seemed due to suggestion to the total number of wrong answers.

	W	C
Numbers of Progressions,	251	226
Numbers of Series,	255	255
Threshold { Limits,	1–18	1–16
Threshold { Mean,	9.5	8.5
Number of Wrong Answers,	1509	952
Ratio of Suggestions to Wrong Answers,	1380 : 1509	783 : 952

There seem to be no facts in connection with these experiments with successive stimuli which do not readily harmonize with the conclusion as to the nature of our tactual perception of two points arrived at two years ago as a result of a series of experiments[1] with simultaneous stimuli in Wundt's institute, viz., that the tactual perception of two points is an assimilation process, based on association, in which visual or motor images are the assimilating, and tactual sensations the assimilated factors. We may repeat again what has been already said, that the local sign is no simple quality of tactual sensations, but rather a relation of association between the different factors, visual, motor

[1] See article in Phil. Stud. referred to above.

and tactual, which enter into the perception image. It is gratifying to find that Solomons[1] has recently come to similar conclusions in regard to the nature of the process involved. Aside from his statement that the process of reducing the threshold by practice, "as well as its general bearing on the origin of cutaneous perceptions, has been considered only speculatively" (which is not literally true), his results, so far as they go, accord for the most part with our own.

The phenomenon which, more than any other, argues against this view seems to be the answer which is sometimes given by the subject, 'different without direction.' But we have found reason for believing that this answer is either an inference from data other than tactual or a sort of illusion which arises in one or other of the following ways: either some non-spatial qualitative difference between the sensations calls up visual or motor images in which this difference appears as spatial, or some suggestion foreign to the immediate experience brings into consciousness such images, and the tactual sensations are wrongly assimilated to them.

[1] Solomons 'Discrimination in Cutaneous Sensations,' PSYCHOLOGICAL REVIEW, Vol. IV., pp. 246–250, especially p. 248.

STUDIES FROM THE HARVARD PSYCHOLOGICAL LABORATORY, IX.

COMMUNICATED BY PROFESSOR E. B. DELABARRE.

The Force and Rapidity of Reaction Movements.

By E. B. Delabarre, Robert R. Logan and Alfred Z. Reed.

INTRODUCTORY NOTE.

It has long seemed desirable to measure, in the taking of reactions, not only the reaction-time itself, but also the degree of pressure used by the subject in reacting and the rapidity with which he contracts his reacting muscles. Reflection on the possible bearing of such measurements upon the matter of individual temperament, and the opportunity afforded by my recent connection with the Harvard Laboratory, led me to devise the apparatus and initiate the experiments herein described. The number of persons on whom measurements were taken is not large, and the number of records taken in the case of each is much smaller than would have been desirable. Yet the time at our disposal has not admitted of greater accomplishment. I publish our results as a beginning of research into this problem, and because they establish the value and suggestiveness of this inquiry, and the facts that a particular average and a particular range of force and of rapidity in reacting are peculiar to each individual. They may possibly prove so typical as to furnish an index to the individual's fundamental characteristics as a whole—a possibility which I plan to discuss in a separate paper on the Study of Temperament.

I cannot refrain from expressing my deep obligation to my two collaborators in this research, who have conducted the experiments with great care and accuracy, and have devoted to the research an amount of time and patience far in excess of what was demanded by their duty to the laboratory. They have jointly contributed the following account of the details of the experiments.

E. B. D.

I. APPARATUS AND METHOD OF RESEARCH.

The apparatus used in these experiments consisted of a revolving drum, on which were recorded a time line indicating hundredths of a second, and parallel to it a signal line, whose deviations indicated the time of reaction and the duration of the reacting movement; a signal key, arranged to break a circuit on giving the signal for reaction; and a special apparatus designed to record the pressure exerted by the subject in reacting, and to close an electric circuit throughout the period during which his muscles are contracting.

The latter piece of apparatus is constructed as follows: A pair of metal jaws project horizontally from a vertical wooden support. They are of such size, and at such a distance apart, as to be conveniently grasped in the hand, or between the thumb and forefinger. The lower one is hinged to its support, so that it can be moved upward by the force of the grasp. The upper one furnishes a firm support for the hand. It is attached, not directly to the wooden support, but to a heavy metal rim, which latter is bolted to the wood. This rim surrounds and supports the upper part of a mercury well, whose flexible leather bottom rests on the lower jaw, and is raised and lowered by its movement. A glass tube projects vertically above the well, and the column of mercury within it stands at a fixed zero point when the lower jaw is in its normal resting position, to which it can be adjusted by a screw. When the hand contracts and the movable jaw is raised, the mercury column is forced upward in the tube to a height varying with the amount of pressure exerted, and with the particular point on the jaw to which the pressure is applied. This point of application of force is different when the jaws are grasped by the whole hand from what it is when their ends are held between thumb and forefinger. Two scales are therefore provided alongside the tube, indicating in kilograms the pressure exerted in either case. An indicator, made of felt, rises with the mercury column, but fits too tightly in the tube to fall back with it. It is pushed back to the zero point, after each experiment, by the weight of a thin rod which, when not in use for this purpose, hangs suspended with its end just within the top of the tube.

This arrangement works with great ease, responding delicately to the slightest movement of reaction. The kilogram divisions are so far apart (for the most part about an inch) that tenths can be easily estimated. Various conditions introduce an error by affecting the normal mercury level, but this error probably does not exceed one or two tenths of a kilogram.

The duration of the reacting movement is recorded on the drum by means of an electric contact made at the instant the movable jaw begins to rise and maintained throughout its upward movement. For this purpose a thin steel lever is provided, turning upon a pivot tight enough to prevent it from falling by its own weight. By means of a screw, its height can be so adjusted, that when at rest its movable end will almost, but not quite, be in contact with the movable end of the lower jaw. The moment the latter begins to move upward, contact is made; the moment it begins to drop back again, contact is broken. In practice it was found that the adjusting screw was unnecessary; for when the lever was pressed directly down upon the jaw its slight recoil due to the friction at its pivot, was sufficient to just raise it from actual contact.

Electrical connections were made as follows: From the batteries to the recording pen, thence to a brake for starting and stopping the drum; from the metal frame of the latter to the signal key and from the signal key to the batteries; thus completing the circuit, closed when the signal key is in normal position, open when the signal is given. The attachment to the brake of the drum is merely a switch to keep the circuit open when the drum is not being used; when the brake is taken off the governor, it is pushed over toward the metal framework; thus closing the circuit at the same time that the drum is made to revolve. Another circuit is made by connections from the signal key to the contact lever of the reaction apparatus, and from the batteries to its movable jaw, in such a way that the circuit, which is interrupted by the giving of the signal, is closed again by the reaction.

The experiments were conducted as follows:

The subject is seated in a chair beside the table upon which the reaction apparatus is placed, with his right elbow resting

upon a cushion; it is important that the subject's position should be as comfortable as possible, in order to avoid fatigue. The subject is then directed to take hold of the jaws with the thumb and forefinger of the right hand, the thumb resting upon the upper one about an inch from the end and the forefinger lightly touching the under part of the lower one at a like distance from the end. It is well, in fact almost necessary, to have two experimenters or operators, one to start and stop the drum and give the signal, the other to record the pressure as indicated by the mercury column, to push the indicator back to zero, to adjust the connection between lever and jaw and to see that the subject keeps his fingers in the same position upon the jaws. The subject being told to what signal he is to react (the sound made by the striking of the signal key) and instructed to think only of making a quick reaction, allowing the force of the reaction to take care of itself, the first operator starts the drum, thus closing the circuit and bringing the recording pen slightly over to the right. When the drum has reacted its normal speed the operator strikes down the signal key, and keeps it pressed down, thus breaking the circuit and throwing the recording pen back to its original position. The subject, upon hearing the signal, reacts by bringing together his thumb and finger, thus forcing the mercury up into the tube and making the circuit by the connection between lever and jaw, which brings the recording pen once more to the right and marks the time required for the reaction. So long as the subject continues his pressure the circuit remains closed, but the moment he ceases, the jaw drops away from the lever, the circuit is broken and the recording pen goes back to the left, thus marking the duration of the reactioning movement. The first operator then stops the drum and releases the key, while the second records the pressure and readjusts the contact lever and the pressure indicator. It is better that the subject keep his eyes closed during each experiment, that his next reaction may not be influenced by seeing what pressure he has exerted. He should also be warned not to grasp the jaws tightly while waiting for the signal, for the slightest raising of the lower jaw brings it into contact with the lever. At the end of the experi-

ment the first operator moves the drum along horizontally, so that the pens will have fresh surface to mark on, and is then ready for the next experiment. It was found that space on the cylinder was saved by moving the drum along after each experiment rather than arranging it to move automatically to one side at the same time that it revolves. From thirteen to sixteen records could be taken on a single roll of smoked paper.

The object of these experiments being to determine the individual peculiarities of the subject's manner of reacting, it was desirable to turn away his attention as much as possible from the reacting movement itself, in order that attention to it might not interfere with its being carried out unconsciously and naturally. In the above described simple reaction experiments this aim was largely secured, since the subject was fully occupied with the endeavor to react as quickly as he could. Besides the simple reactions, however, another series was taken of associative reactions, in the hope that the greater degree of concentration required in the search for an association would withdraw the attention still more fully from the reaction movement. The time of association itself was recorded. But since its duration was not the prime object of investigation, the operators contented themselves with the accuracy obtainable by the endeavor on the part of signal giver and reactor to speak their words as simultaneously as they could with the pressing of their keys; and used no special apparatus to make this simultaneity absolute.

II. EXPLANATION OF TABLES.

The subjects were fifteen in number. They are designated at the head of the tables by letters of the alphabet. On each subject two series of experiments were performed. Series I. deals with simple reactions, Series II. with association reactions. In each series the results are set down, in the order in which the experiments were performed, in three columns. Column R gives the reaction time (simple or associated) expressed in thousandths of a second (σ). Column D gives the 'duration time;' the period, that is to say, during which the pressure of the reacting muscles increases in intensity, and is expressed also in thousandths of a second. Column P gives the maximum pressure

expressed in tenths of a kilogram. In all three columns alike, the last digit of the tabulated quantity was estimated by the eye, the remaining digits being recorded by mechanical means.

In addition to these three columns of figures, which represent the direct results of experimentation, the pressure index of each experiment has been divided by the corresponding duration time, and the quotient expressed accurately to two places of decimals. Since P is expressed in tenths of a kilogram and D in thousandths of a second, this quotient shows the average number of hectograms (or, considered not as a fraction but as a whole number, the average number of grams) of pressure exerted by the contracting muscles during each σ of their contraction. It is therefore an index to the rapidity with which the movement of contraction is executed.

Finally the average and the average variation of columns R, P and $\frac{P}{D}$, have been worked out.

An asterisk (*) in the first three columns indicates that owing to imperfections in the mechanical records the numerical result is missing; or, when figures are given, that they are not absolutely trustworthy. The asterisk has been inserted whenever there was the slightest question as to the exact figures. In every other case the operators feel confident of the entire accuracy of their results, except in so far as the final digits are subject to errors in assessment.

The letter A placed before any quantity signifies that that quantity has not been reckoned in making up the average and average variation of the column; and the letter A placed outside, on the left of the columns of figures, signifies that none of the quantities, on the line on which it stands, have been reckoned in making up these averages. The A is usually added in case of possible inaccuracy marked by the asterisk; and is also employed when one quantity or set of quantities shows a striking and unexplained divergence from the other quantities in that series.

It will also be observed that when the asterisk shows possible inaccuracy in a duration time or pressure index, the corresponding $\frac{P}{D}$ relation has not, as a rule, been worked out.

A.

	Series I.				Series II.			
	R	D	P	$\frac{P}{D}$	R	D	P	$\frac{P}{D}$
	A331	332	63	0.19	534	792	37	0.05
	260	350	69	0.19	769	419	68	0.16
	275	490	79	0.16	761	453	62	0.14
	234	302	50	0.17	730	524	57	0.11
	173	482	97	0.20	886	505	70	0.14
	231	331	61	0.18	690	554	75	0.14
	225	339	68	0.20	940	519	63	0.12
	180	373	43	0.11	827	450	59	0.13
	221	404	59	0.15	900	572	71	0.12
	A371	340	67	0.20	753	590	70	0.12
	224	312	64	0.21	636	671	77	0.11
	220	345	63	0.18	614	543	88	0.16
	264	336	64	0.19	726	484	82	0.17
A	187	*371	68					
Average . . .	228		65	0.18	751	544	68	0.13
Av. Var. . . .	22		8	0.02	89	78	9	0.02

B.

	Series I.				Series II.			
	R	D	P	$\frac{P}{D}$	R	D	P	$\frac{P}{D}$
	225	276	54	0.20	802	321	66	0.21
	251	*287	63	0.22	*	*	66	
	226	258	55	0.21	*	*	59	
	219	337	74	0.22	726	250	52	0.21
	209	346	85	0.25	675	285	54	0.19
	175	238	51	0.21	775	294	58	0.20
	207	165	37	0.23	669	297	54	0.18
	202	229	47	0.21	683	218	48	0.22
	290	200	46	0.23	1086	274	45	0.16
	A393	232	52	0.22	840	393	70	0.18
	181	247	53	0.21	998	325	52	0.16
	185	196	45	0.23	740	292	58	0.20
	216	196	42	0.21	508	222	51	0.23
	236	201	47	0.23				
	225	*189	44	0.23				
Average . . .	218		53	0.22	773		56	0.19
Av. Var. . . .	21		9	0.01	116		6	0.02

C.

	Series I.				Series II.			
	R	D	P	$\frac{P}{D}$	R	D	P	$\frac{P}{D}$
	146	263	105	A0.40	510	525	A+110*	
	220	366	88	0.22	430	667	113	0.17
	152	474	A+120*		280	448	111	0.25
	207	370	115	0.31	360	440	110	0.25
	201	307	83	0.27	437	445	105	0.24
	268	294	86	0.29	335	445	91	0.20
	154	320	88	0.27	285	455	84	0.18
	146	298	87	0.29	A145	670	107	0.16
	225	312	86	0.28	500	575	100	0.18
	134	273	75	0.27	410	715	95	0.13
	170	500	*120	0.24	705	440	98	0.22
	97	486	101	0.21	457	350	70	0.20
					390	410	85	0.21
Average .	177		94	0.27	425		95	0.20
Av. Var. .	37		12	0.02	81		11	0.03

D.

	Series I.				Series II.			
	R	D	P	$\frac{P}{D}$	R	D	P	$\frac{P}{D}$
	115	124	*		1062	395	79	0.20
	A390	248	30	0.12	1524	410	67	0.16
	180	170	39	0.23	1167	412	73	0.18
	160	145	35	0.24	1435	360	62	0.17
	145	223	47	0.21	1760	340	65	0.19
	161	235	49	0.21	1174	350	60	0.17
	A220*	A240*	32		1080	320	64	0.20
	154	235	50	0.21	1376	327	69	0.21
	150	175	35	0.20	1610	308	67	0.21
	162	278	43	0.15	1090	350	74	0.21
	148	263	54	0.21	1667	324	68	0.21
	168	267	44	0.16	990	345	77	0.22
	166	300	58	0.19				
	167	315	60	0.19				
	145	298	52	0.17				
Average .	155		45	0.19	1328		69	0.19
Av. Var. .	12		8	0.03	234		5	0.02

E.

	Series I.				Series II.			
	R	D	P	$\frac{P}{D}$	R	D	P	$\frac{P}{D}$
	185	358	54	0.15	871	380	51	0.13
	143	327	60	0.18	A987*	A456*	52	
	159	278	49	0.18	A1588	371	55	0.15
	155	287	50	0.17	915	279	36	0.13
	150	224	46	0.21	737	350	50	0.14
	178	178	40	0.22	955	409	42	0.10
	140	175	A35*		694	A870	47	
	147	219	42	0.19	706	297	54	0.18
	190	215	40	0.19	570	282	50	0.18
	191	191	41	0.21	664	283	52	0.18
	167	194	41	0.21	654	253	46	0.18
	190	191	37	0.19	*	*	45	
	157	150	33	0.22	*	*	51	
					624	312	48	0.15
					A1293	484	66	0.14
Average .	166		44	0.19	739		50	0.15
Av. Var. .	17		6	0.02	104		4	0.02

F.

	Series I.				Series II.			
	R	D	P	$\frac{P}{D}$	R	D	P	$\frac{P}{D}$
	220	200	59	0.29	710	630	67	0.11
	200	170	53	0.31	1110	340	54	0.16
	200	295	58	0.20	1035	375	68	0.18
	195	222	50	0.23	970	500	75	0.15
	186	366	56	0.15	900	335	70	0.21
	125	315	52	0.17	1150	337	67	0.20
	180	320	70	0.22	830	361	69	0.19
	140	320	61	0.19	981	328	63	0.20
	115	260	69	0.27	1760	400	58	0.14
	115	315	54	0.17	1712	340	61	0.18
	165	280	56	0.20	880	365	76	0.21
	160	265	54	0.20				
Average .	167		58	0.22	1094		66	0.18
Av. Var. .	30		5	0.04	246		5	0.02

G.

	Series I.				Series II.			
	R	D	P	$\frac{P}{D}$	R	D	P	$\frac{P}{D}$
	328	412	56	0.14	1460	481	83	0.17
	252	473	54	0.11	1098	469	73	0.16
	314	223	34	0.15	A1105*	A400*	50	
	231	463	63	0.14	A1710*	A420*	50	
	210	290	37	0.13	862	383	55	0.14
	243	270	37	0.14	800	317	35	0.11
	222	263	37	0.14	822	332	37	0.11
	230	256	36	0.14	1073	328	37	0.11
	207	467	63	0.13	780	549	34	0.06
	216	367	57	0.16	599	834	32	0.04
	264	350	51	0.15	828	489	35	0.07
	220	202	34	0.17	848	547	36	0.07
	360	234	33	0.14				
	193	352	41	0.12				
Average .	249		45	0.14	917		46.5	0.10
Av. Var. .	39		10	0.01	176		13	0.04

H.

	Series I.				Series II.			
	R	D	P	$\frac{P}{D}$	R	D	P	$\frac{P}{D}$
	A300	435	70	0.16	*	*	62	
	160	350	70	0.20	1100	556	98	0.18
	126	300	61	0.20	1304	438	95	0.22
	155	360	80	0.22	895	480	84	0.17
	165	310	65	0.21	700	494	60	0.12
	162	292	50	0.17	855	343	70	0.20
	218	305	49	0.16	590	400	77	0.19
	130	360	75	0.21	897	408	61	0.15
	142	340	70	0.21	1333	355	45	0.13
	122	400	82	0.20	790	618	90	0.15
	144	493	88	0.18	1875	607	58	0.10
	165	463	99	0.21				
	170	400	84	0.21				
	155	413	88	0.21				
Average .	155		74	0.20	1034		73	0.16
Av. Var. .	17		11	0.02	275		15	0.03

J.

	Series I.				Series II.			
	R	D	P	$\frac{P}{D}$	R	D	P	$\frac{P}{D}$
A	350	294	43		1203	621	88	0.14
	293	401	53	0.13	2271	577	67	0.12
	183	300	43	0.14	1300	542	67	0.12
	249	347	47	0.14	1423	573	68	0.12
	173	403	56	0.14	A556	A975	74	
	190	430	60	0.14	1222	724	77	0.11
	147	418	49	0.12	1190	561	83	0.15
	188	457	53	0.12	1594	586	62	0.11
	299	432	63	0.15	1490	514	59	0.11
	200	475	60	0.13	1097	A386*	67	
	180	487	57	0.12	1009	486	69	0.14
	172	470	54	0.12				
	194	481	61	0.13				
Average .	206		55	0.13	1380		71	0.12
Av. Var. .	37		5	0.01	252		7	0.01

K.

	Series I.				Series II.			
	R	D	P	$\frac{P}{D}$	R	D	P	$\frac{P}{D}$
	227	321	A38	Ao.12	1391	331	71	0.21
	229	403	69	0.17	995	359	73	0.20
	158	355	69	0.19	984	372	72	0.19
	181	340	66	0.19	1731	320	64	0.20
	181	288	54	0.19	1818	348	73	0.21
	204	256	59	0.23	1131	354	61	0.17
	291	341	69	0.20	1217	408	75	0.18
	167	232	53	0.23	1152	326	69	0.21
	173	301	58	0.19	1337	363	77	0.21
	169	320	57	0.18	1494	291	63	0.22
	175	302	60	0.20	*	A510	65	
	165	266	53	0.20	1037	355	67	0.19
	180	267	58	0.22	1113	353	73	0.21
	174	207	46	0.22				
Average .	191		59	0.20	1283		69	0.20
Av. Var. .	27		6	0.01	226		4	0.01

L.

	Series I.				Series II.			
	R	D	P	$\frac{P}{D}$	R	D	P	$\frac{P}{D}$
	143	464	42	0.09	629	293	49	0.17
	127	238	32	0.14	384	219	38	0.17
	153	264	41	0.16	481	378	54	0.14
	188	134	20	0.15	415	292	40	0.14
	166	236	37	0.16	402	411	47	0.11
	153	244	40	0.16	602	336	47	0.14
	164	230	47	0.20	473	401	43	0.11
	173	156	35	0.22	540	290	41	0.14
	151	205	43	0.21	455	379	57	0.15
	110	177	37	0.21	703	307	42	0.14
	150	180	35	0.19	575	373	51	0.14
	163	217	41	0.19	952	260	44	0.17
	*	*	24		731	266	46	0.17
Average .	153		37	0.17	565		46	0.15
Av. Var. .	14		6	0.03	124		4	0.02

M.

	Series I.				Series II.			
	R	D	P	$\frac{P}{D}$	R	D	P	$\frac{P}{D}$
	176	437	87	0.20	1107	307	66	0.21 (1)
	180	294	60	0.20	1058	321	57	0.18 (1)
	250	205	40	0.20	881	944	59	0.06 (2)
	165	170	39	0.23	1110	755	57	0.08 (2)
	180	174	41	0.24	1200	795	52	0.07 (2)
	163	295	60	0.20	700	250	50	0.20 (1)
	190	162	43	0.27	*	*	54	
	175	123	33	0.27	1310	520	58	0.11 (2)
	215	226	47	0.21	*	*	45	
	170	154	40	0.26	1345	1007	63	0.06 (2)
	195	277	60	0.22	803	1165	45	0.04 (2)
	150	251	57	0.23	1120	420	56	0.13 (2)
	165	225	49	0.22				
	180	272	60	0.22				
Average .	182		51	0.23	1063		55	0.11
Av. Var. .	17		11	0.02	162		5	0.05
								(1) (2)
Average .								0.20 0.08
Av. Var. .								0.01 0.02

N.

	Series I.				Series II.			
	R	D	P	$\frac{P}{D}$	R	D	P	$\frac{P}{D}$
A	205	320	47	0.15	907	830	71	0.09
	280	355	51	0.14	*	*	70	
	180	247	66	0.27	905	550	61	0.11
	280	216	65	0.30	805	530	75	0.14
	185	255	56	0.22	800	520	59	0.11
	336	274	58	0.21	575	580	69	0.12
	222	232	63	0.27	565	545	66	0.12
	219	261	74	0.28	460	615	77	0.13
	205	141	56	0.39	535	617	65	0.11
	263	186	62	0.33	505	485	71	0.15
	256	202	61	0.30	700	660	80	0.12
	204	182	64	0.35	662	612	76	0.12
	209	170	57	0.34	625	675	70	0.10
					620	399	65	0.16
					690	360	52	0.14
Average .	226		60	0.28	668		68	0.12
Av. Var. .	30		6	0.06	114		6	0.01

O.

	Series I.				Series II.			
	R	D	P	$\frac{P}{D}$	R	D	P	$\frac{P}{D}$
	270	433	103	0.24	614	563	80	0.14
	380	338	61	0.18	700	360	69	0.19
	395	386	71	0.18	800	517	78	0.15
	170	360	74	0.21	685	*	81	
	175	370	83	0.22	817	466	78	0.17
	225	392	79	0.20	937	570	83	0.15
	179	440	85	0.19	760	430	62	0.14
	290	400	78	0.19	400	360	61	0.17
	210	335	71	0.21	265	521	79	0.15
	204	255	58	0.23	A1795	*	73	
	190	215	50	0.23	820	360	68	0.19
	230	165	50	0.30				
	205	200	58	0.29				
Average .	240		71	0.22	680		74	0.16
Av. Var. .	57		12	0.03	152		7	0.02

P.

	Series I.				Series II.			
	R	D	P	$\frac{P}{D}$	R	D	P	$\frac{P}{D}$
A	320	225	45	0.20	1240	506	40	A0.08
	237	180	31	0.17	1176	317	43	0.14
	207	270	34	0.13	1057	242	39	0.16
	200	187	32	0.17	1026	A1080	32	
	215	152	32	0.21	1195	234	33	0.14
	215	165	30	0.18	1491	255	*	
	275	333	36	0.11	1380	212	31	0.15
	265	175	*20+	0.14	990	196	35	0.18
	210	195	33	0.17	1796	350	43	0.12
	222	166	31	0.19	A2320	270	43	0.16
A	199	385	32	A0.08	1730	265	37	0.14
	213	206	32	0.16	870	345	45	0.13
	270	287	47	0.16				
	231	276	44	0.16				
Average .	230	216	34	0.16	1268		38	0.15
Av. Var. .	21	50	4	0.02	241		4	0.01

III. RESULTS.

The present operators have had but little time to devote to the interpretation of their results. But even so, several facts can be pointed out, which certainly do not prove laws, but which as certainly are suggestive and point the way to development by future workers in this fascinating research.

1. First, what perhaps we should have been led *a priori* to expect, is corroborated. Divide P by D, the pressure index by the duration time, and the resultant quotient, representing the rapidity of contraction of the reacting muscles, tends, for the same individual and the same series, to be constant. Look, for instance, examining the tables almost at random, at B I; where the pressure varies from 37.5 up to 85, the duration varies so uniformly in proportion, that the quotients $\frac{P}{D}$ are all found within a range of 0.05—from 0.20 to 0.25; where there was an average variation in the case of P. of 9, there is an average

TABULAR SUMMARY.

SUBJECT AND SERIES.	PRESSURE.					RAPIDITY.				
	Max.	Min.	Av.	Av. Var.	Med.*	Max.	Min.	Av.	Av. Var.	Med.*
A I	97	43	65	8	64	.21	.11	.18	.02	.19
II	88	37	68	9	70	.17	.05	.13	.02	.13
B I	85	37	53	9	51	.25	.20	.22	.01	.22
II	70	45	56	6	54	.23	.16	.19	.02	.20
C I	115	75	94	12	88	.31	.21	.27	.02	.27
II	113	70	95	11	100	.25	.13	.20	.03	.20
D I	60	30	45	8	45	.24	.12	.19	.03	.20
II	79	62	69	5	68	.22	.16	.19	.02	.20
E I	60	33	44	6	42	.22	.15	.19	.02	.19
II	66	36	50	4	51	.18	.10	.15	.02	.15
F I	70	50	58	5	55	.31	.15	.22	.04	.20
II	76	54	66	5	67	.21	.11	.18	.02	.18
G I	63	33	45	10	39	.17	.11	.14	.01	.14
II	83	32	46	13	37	.17	.04	.10	.04	.11
H I	99	49	74	11	70	.22	.16	.20	.02	.21
II	98	45	73	15	70	.22	.10	.16	.03	.16
J I	63	43	55	5	54	.15	.12	.13	.01	.13
II	88	59	71	7	68	.15	.11	.12	.01	.12
K I	69	46	59	6	58	.23	.17	.20	.01	.20
II	75	61	69	4	71	.22	.17	.20	.01	.20
L I	47	20	37	6	37	.22	.09	.17	.03	.17
II	57	38	46	4	46	.17	.11	.15	.02	.14
M I	87	33	51	11	48	.27	.20	.23	.02	.22
II	66	45	55	5	55	.21	.04	.11	.05	.09
N I	74	47	60	6	61	.39	.14	.28	.06	.28
II	80	52	68	6	70	.16	.09	.12	.01	.12
O I	103	50	71	12	71	.30	.18	.22	.03	.21
II	83	61	74	7	78	.19	.14	.16	.02	.15
P I	47	30	34	4	32	.21	.11	.16	.02	.16
II	45	31	38	4	39	.18	.12	.15	.01	.14

variation in the case of $\frac{P}{D}$ of only 0.01.

2. Secondly, there are well marked differences in rapidity between the different individuals, and between the two series of the same individual. These differences are of two kinds:

*In column 'Med.' is given the 'Median' as distinguished from the average.

First, absolute. The average value of $\frac{P}{D}$ in Series I. for subject J is 0.13; for N it is 0.28. J's maximum is 0.15; N's minimum is 0.14. They then just overlap; but J's minimum is 0.12, and N's maximum 0.39. The second way in which individuals differ is in this range of variation in value of $\frac{P}{D}$. Compare the comparative constancy of subjects J, H, B and M in Series I., with the extraordinary freedom from law and the trammels of consistency, which C, F and N display.

3. Thirdly, it is observable that although the two series overlap each other a great deal, yet in no case is the maximum value of $\frac{P}{D}$ in Series II. greater than in Series I.; and in no case is the average value in Series II. the greater. How far this uniform lessening in rapidity in Series II. is due to untraced workings of consciousness, and in how far to the frequently observed tendency of the reagent in Series II. to increase his duration time by starting to react before he had really found his associated idea, is a question for subsequent investigation to determine.

4. Although the range of variation in pressure is larger than in rapidity, yet similar observations can be made for it also. The degree of pressure exerted and the range of its variation are characteristic of the individual. Each has his own special tendencies and his own limits of variation, differing from those of the others. Some exert little pressure in reacting, some much. Some are fairly constant, while others range over varying pressures whose extremes are widely apart. In the majority of cases a comparison of the two series shows for each individual a manner of reacting similar in both series, even though the absolute values of average and limits may differ in the two. This correspondence of the two series, in spite of the small number of experiments entering into each, furnishes strong evidence that we here gain insight into fundamental characteristics of the individual.

5. If we compare Series I. with Series II., it will be noted that in every case but one the average pressure is greater in

Series II.; whereas it has been seen that the rapidity is greater in Series I. Both facts might easily be due to the above mentioned increase in duration of the reacting movement.

Many further questions readily present themselves. What relations, if any, has R in either series to P or to D or to $\frac{P}{D}$? So far as this investigation can show, there would seem to be no fixed relation between them. Will further research confirm the hypothesis so strongly supported by the experiments here reported, namely, that everyone has his own personal and comparatively constant manner of reacting, his own usual degree of rapidity and of force in making movements when his attention is not occupied with the way in which he carries them out, and his own limits of possible variation from their average? Under what circumstances does he vary in particular ways within these limits? What is the cause of these individual tendencies and differences? Are they affair of mood, or of temperament, or of other causes? Are the values found in simple reactions, or in associative reactions, or in both together, or even in some other yet uninvestigated form, more characteristic of the individual? These questions, and many more which easily arise, can be answered only by spending much time and labor in collecting more data along these lines; and still more time and labor in digesting and interpreting the data so obtained.

After-Sensations of Touch.

By Frank N. Spindler.

The subject of investigation for this series of experiments has been what are generally called After-Images of Touch, but which should more properly be called After-Sensations of Touch. The word Image applies well to the case of after-effects of sight, but would seem to have no particular application to the after-feeling of a sensation of touch.

We are all more or less familiar with after-sensations of all sorts, but the very fact that these after-sensations are so com-

mon and often undifferentiated, makes them very difficult to study with any accuracy. Most of us have never attended to an after-sensation of touch or pressure long enough to be certain of its quality or duration. These sensations are all merged into the general and common muscular and organic feelings and hence pass unnoticed.

An after-sensation of touch or pressure by no means implies any illusion as to the pressure still continuing. The experiment is often tried of blindfolding a person and then pressing a coin upon the forehead; on removing it the subject will not for some time notice that it is gone. We often feel a pen behind the ear even after it is no longer there, or think our hat is on when it is off. But in all these cases a little analysis would enable the subject to distinguish between the actual touch or pressure and the after-sensation. The latter is qualitatively, as well as quantitatively, distinct from the actual feeling of the stimulation.

The literature on the subject of after-sensations of touch is meagre. Many psychologists make no mention whatever of the subject, while others refer to it but briefly.

James, speaking in general of after-sensations of all kinds, says that the nervous matter has an inertia and elasticity, a certain time of stimulation is necessary to excite any kind of a sensation, a certain time then conversely might be expected to be necessary for the sensation to fade away. Sensations then outlast, for a little time, the objective stimuli which occasion them. They show that profound rearrangements and slow settlings into a new equilibrium are going on in the neural substance.

Baldwin, speaking of after-sensations in a general way, says likewise: "There is a vibratory persistence, in the nervous organism, of peripheral shocks, which tends to continue the central process and its accompanying mental state. And the same residum or after-effect is also probably a mental necessity, since time is needed for the shifting movements of attention in its transition to new experiences; during this period there is nothing to drive the former experience from consciousness and it persists a noticeable time."

If we turn to Külpe we find an attempt to give a more particular description of the actual mechanism of after-sensation. He fails, however, to distinguish between two completely distinct types of after-effect in the field of contact and pressure. A light, quick touch on some portions of the skin occasions not only an ordinary sensation of contact, but also, after a brief interval—a second or less—a second quick pulse of sensation. To observe this, the stimulating object must be immediately removed, else the secondary sensation will be drowned in the continuing primary sensation. This effect, however, is not at all analogous to the after-images, positive and negative, which occur in vision and in other senses, due to continuance of the excitation in the peripheral organ, or to a restoration of its equilibrium, after the stimulation has ceased. Yet the sense of touch presents after-sensations of this kind also, and these Külpe does not mention. His theoretical discussion as to whether the after-sensation is due to a double path of conduction through the spinal cord, or to the existence and coöperation of centrifugal with centripetal sensory fibers, applies only to the brief secondary sensation after brief stimulation—the first type of after-effect mentioned above. The present study, however, has been only of the after-sensations of the other type—the varying sensations which follow the cessation of a more or less prolonged stimulation of the skin by means of pressure.

To investigate these weights were used of from 25 up to 1,000 grams. These were placed upon a brass holder with a round base seven-eighths of an inch in diameter, covered on the bottom with paper, so as to prevent differences of temperature between the metal and the skin.

The weights were applied to the backs of the hands, these being the most convenient parts of the body to practice on and also the parts especially susceptible to pressure sensations. They were left in contact during lengths of time varying from five seconds to ten minutes. Record was then made of the different sensations following their removal.

These experiments, however, labor under the disadvantage of being entirely dependent upon introspection for results, and also of dealing with very vague, indefinite and irregular phe-

nomena. It is difficult to judge accurately the presence and nature of these after-sensations; to distinguish them from the subjective sensations easily discoverable whenever attention is directed to the skin; to analyze them out from among the mass of organic feelings with which they so easily fuse; to determine the moment of their appearance or disappearance.

There is a marked difference in subjects as to their power to discriminate these after-sensations. Some are very sensitive to them and get all sorts of after-sensations, while others cannot get any whatever, although their failure is probably due to lack of practice in introspection.

The physical and mental condition of the subject has a marked effect. If the subject is tired or sick, or gloomy, the after-sensations are dull and shorter continued than when the subject is well and cheerful. In the writer's own introspection he found that when he was melancholy and in a state of general depression or exhaustion, with languid circulation and slow pulse beat, there would be hardly any after-sensations experienced at all, even after long stimulation, while in a more cheerful and active state of mind and body the after-sensations would be vivid and long continued even after brief stimulation. This difference is doubtless due, in part, to the fact that, when depressed, one is skeptical of any feeling or sensation of any kind. In view of these difficulties it is not surprising that the most striking characteristic of the results we have gained consists in their extreme irregularity and apparent inconsistency. It seems impossible to establish any definite and consistent relation between the time of stimulation and the duration of the after-sensation, or between the degree of pressure and the duration or vividness of the after-effect. Such indefinite formulation as is possible, however, will be attempted in the following account of our results.

1. SHORTEST DURATION OF STIMULATION NECESSARY FOR THE APPEARANCE OF AN AFTER-SENSATION.

It was found that 100 gms. for 5 seconds gave no perceptible after-sensation, but only a feeling of relief on removal of the weight; 25 gms. for 1 minute, gave a strong after-sensation

lasting 3 minutes; 150 gms. for 5 seconds, however, gave a weak after-sensation in the shape of a feeling of warmth and contraction, lasting about ten seconds; 150 gms. for 5 seconds then was about the lowest threshold as to time of stimulation.

2. INTERVAL BETWEEN END OF STIMULATION AND APPEARANCE OF AFTER-SENSATION.

There occurs always a certain interval between the removal of the stimulus and the conscious beginning of the after-sensations. The average duration of all these intervals, in this series of experiments, was about 36½ seconds. Their length varies with the time of stimulation, as will be seen from the following table for different stimulations of from 10 seconds up to 10 minutes. Each interval here given is an average from several trials:

TIME OF STIMULATION.	INTERVAL.	DURATION OF AFTER-SENSATION.
10 seconds.	5 seconds.	30 seconds.
30 "	10 "	4⅓ minutes.
1 minute.	39 "	5 "
2 minutes.	47½ "	4½ "
3 "	1 minute, 22½ seconds.	5 " and 18 seconds.
5 "	44 seconds.	9 "
10 "	44⅓ "	10 "

There appears to be but little relation here, either between the duration of the stimulation and the length of the interval between end of stimulation and after-sensation, or between the length of the stimulation and the duration of the after-sensation itself. The intervals, however, seem to increase up to stimulations of 3 minutes' duration and then to drop again, being about the same for a 5 or a 10 minute stimulation as for a 2 or a 3 minute one.

3. DURATION OF THE AFTER-SENSATION.

The absence of any fixed dependence of the duration of the after-sensation upon the time of stimulation is also apparent

from the above table. For any time of stimulation from 1 to 3 minutes the after-sensation seems to be about the same in duration, that is about 5 minutes; while for a 5 to 10 minute stimulation the average after-sensation lasts 10 minutes.

It is absolutely impossible to have these results exactly accurate, for the after-sensations of touch fade away so gradually, and it is so easy to call them back even an hour after first ceasing to notice them, that to say absolutely when they stop is not possible. Under the most careful attention, however, they seem not to last more than 10 minutes at most. Their later recurrence is apparently not actually a matter of direct after-effect, but rather a result of turning attention to the skin, which always easily arouses subjective sensations, without previous special stimulation.

The most that we can say is that there seems to be a tendency for a longer stimulation to produce a longer after-sensation, but not a relatively longer one. For example, stimulations of 1 minute produce after-sensations averaging 5 minutes in duration, while stimulations of 10 minutes average 10 minutes; those of 5 minutes produce after-sensations averaging 9 minutes, and those of 2 or 3 minutes produce after-sensations averaging 5 minutes.

There seems then to be a limit to the time the after-sensation can naturally continue, and a longer stimulation cannot produce a proportionately longer after-sensation. Indeed, there may be a tendency in a long continued touch or pressure to deaden the nervous sensibility, as there is no reason to suppose nerve substance to be capable of indefinite reaction without exhaustion and lethargy.

It might be supposed that there would be a more definite relation between the heaviness of the weights used and the duration and strength of the after-sensations. We might expect perhaps that the heavier the weight, the longer and more vivid the after-sensation would be. But here also the results are very irregular, as the following tables will show.

The weights of from 150 to 500 grams seem to produce the longest after-sensations, but even this is contradicted in the cases of stimulation of 5 minutes' duration, where 100 grams

Weight.	Time of Pressure.	Average Duration of After-Sensation.
100 gms.	1 minute.	4 minutes and 48 seconds.
150 "	1 "	3 "
200 "	1 "	8 " " 30 "
500 "	1 "	7 " " 12 "
1000 "	1 "	6 "
100 gms.	2 minutes.	4 minutes.
150 "	2 "	5 " and 6 seconds.
200 "	2 "	4 " " 36 "
500 "	2 "	7 "
1000 "	2 "	5 "
100 gms.	3 minutes.	5 minutes and 18 seconds.
500 "	3 "	10 "
1000 "	3 "	7 " " 30 "
100 gms.	5 minutes.	10 minutes.
150 "	5 "	8 "
200 "	5 "	8 " and 18 seconds.
500 "	5 "	8 "
1000 "	5 "	7 " " 30 "
100 gms.	10 minutes.	7½ minutes.
150 "	10 "	10 "
200 "	10 "	10 "
1000 "	10 "	7½ "

for 5 minutes gives a 10 minute after-sensation. These results may seem erratic, but, as has been said, the introspection necessary to distinguish the cessation of an after-sensation of touch is extremely difficult. The effect is one that fades gradually away and the exact moment when the sensation is dropped from consciousness is hard to decide.

It is noticeable, however, that 1,000 grams in every case produced a shorter after-sensation than did 500 grams, or even 150 or 200 grams, for the same time of pressure. This would agree with Bain's assertion that the papillæ touched lightly give a greater reaction than when a heavy weight is applied, for the latter seems to deaden the conductibility of the nerves and gives comparatively little sensation.

4. QUALITY AND VARIETY OF THE AFTER-SENSATIONS.

To the writer's own introspection an after-sensation of touch assumes simply a feeling of contraction as if the spot were painted with collodion or mucilage. This contracted feeling was generally strongest at from 2 to 3 minutes after the removal of the stimulus and then faded gradually away. But other subjects got more of a richness and variety of feeling out of their after-sensations than this. They got the feeling of contraction of the skin, but also temperature sensations, both warm and cold, external smarting sensations, and also a deeper dull ache. Let me give a subject's account of an after-sensation. 100 grams were applied for 5 minutes. After a negative interval of 40 seconds after removal of weight a touch sensation was felt. At 1 minute, warmth; 1 minute 20 seconds, very warm with touch sensations; 2 minutes, pain with heat; 3 minutes, touch again more prominent, and also a feeling of contraction in the spot, the pain and touch alternate; 5 minutes, principally pain; 7 minutes, pain, unpleasant; 8 minutes, pain in whole hand, contracted feeling in the spot brought out on bending the hand; 9 minutes, pain fades; 10 minutes, no sensation.

Here we have a fairly constant touch sensation together with alternating pain and temperature, and finally the pain alone persisting and then fading. This subject sometimes had sensations of cold as well as of heat.

Another experiment, on another subject, gave the following results: One hundred grams were applied to back of right hand for 1 minute; negative interval of 1 minute. After one minute, a slight cold feeling felt in spot; after 2 minutes, colder; after 2½ minutes, warmth; after 4 minutes, pain deep seated in hand; after seven minutes, vague discomfort; after 9 minutes, cold; after 9 minutes 20 seconds, no sensation.

In this subject then we see temperature sensation in waves, also a touch sensation as of contraction of the skin and also a deeper seated pain.

Another subject got even a more remarkable lot of feelings out of the after-sensations.

In one experiment, for example, 100 grams were applied for 5 minutes, negative interval of 50 seconds. After 1 minute, a

drawing sensation; after 1 minute and 20 seconds, itching; after 1 minute and 50 seconds, deep, dull ache; after 2 minutes, warm, feverish sensation; after 2½ minutes, sharp pain, with waves of heat; after 3½ minutes, dull ache; after 4 minutes, sharp toothache pain; after 5 minutes, decided dull ache; after 6 minutes, decided dull ache; after 7 minutes, decided dull ache; after 8 minutes, intervals of no sensation; after 8½ minutes, heat; after 9 minutes, slight heat; after 10 minutes, no sensation. There are here two surface sensations—one of touch, the other of smarting; and also a deeper dull ache—besides the temperature sensations, which were pronounced sometimes cold and sometimes hot.

This subject had indeed a remarkable faculty of getting sensations. In several instances with him the weight was placed on one hand and then the attention was fixed upon a symmetrical spot on the other hand. The spot on the hand actually pressed would force itself upon the attention after an interval and give sensations of touch, heat, etc., but the hand where the spot was attended to without previous pressure would, after a few minutes, go through the same series of sensations as the actually stimulated spot, although not at first so intensely. But if this subject kept his attention turned to a spot on the hand its sensations become in time exceedingly disagreeable.

As for the writer's own introspection in regard to an actually stimulated spot on one hand and a symmetrical spot on the other hand simply attended to, the actually stimulated spot seemed to yield a stronger after-sensation than the sensation which arose in the spot merely attended to, and of a different quality of sensation, yet so similar that some might call it merely a difference of degree.

The very fact, however, that by turning our attention to a spot we can cause lasting and, indeed, even painful sensations, seemingly as strong as those given by actually stimulated spots, and to some subjects qualitatively the same, increases enormously the difficulty of investigating the actual after-effects of stimulation themselves. This difficulty we have kept in mind, and have eliminated, so far as possible, its influence in the records whose results are above tabulated.

We may sum up the results of this study very briefly, as follows:

The minimal time of stimulation which will yield an after-sensation of the kind under investigation is about 5 seconds, with a pressure of 150 grams.

The relation between the duration of stimulation and the length of the interval which elapses before the appearance of the after-sensation is very irregular. The intervals increase up to stimulations of about 3 minutes, and then again decrease.

The duration of the after-sensation increases with the duration of stimulation, though without any discoverable regularity. It is possible that there is a limit to this increase—a possibility which we have not subjected to full investigation.

The longest duration of after-sensations is given by pressures of from 150 to 500 grams. Above and below these limits of pressure the duration decreases.

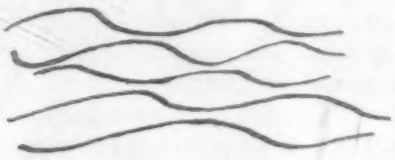
In quality the after-sensations are very variable.

The writer could discover no waves in his own after-sensations, but only a steady persistent feeling of contraction. Other subjects, however, experienced waves of heat, of pain, etc.; but they also in most cases felt a steady persistent underlying touch or contraction sensation, lasting through the dull aches, the smarts, and the heat or cold.

DISCUSSION AND REPORTS.

THE COLOR-VISION OF APPROACHING SLEEP.

The experiment of Mr. Havelock Ellis on the color producing properties of mescal (noticed in the September number of this REVIEW), gives me occasion for describing an experience of my own which I have not seen referred to by others. It sometimes happens to me to fall asleep over a book, and upon such occasions I sometimes catch a play of various colors upon the printed page. The first time that this occurred I was very nearly sound asleep, although my eyes were still open. The colors were very brilliant, and they presented a rather regular wavy pattern in red and green, something like this:



I should mention that I am rather more apt than most people to go to sleep piecemeal, if I may express it so. I have more than once continued reading aloud, so as to be understood, for a full sentence or two after I had absolutely lost consciousness of what I was doing. Upon the occasion I refer to I was certainly waked up suddenly from distinct slumber by the startlingly brilliant colors on the page before me. They were so interesting, as soon as I caught sight of them, that they caused me to become at once wide awake, and then they immediately disappeared. This experience of seeing very pure and brilliant colors I have had only half a dozen times in all,—the first case occurring about a year ago. But since I have become familiar with the phenomenon, I can see any evening, as soon as I begin to get sleepy, that the page before me is broken up into largish patches of violet and a complementary yellowish green, neither brilliant nor saturated. If the page is a newspaper, or other irregular surface, it is evident that the violet color covers its shadier portions.

The occasions when the phenomenon was distinctly different from this, of which I have notes made at the time, are the following:

The page had rather even, smaller patches of brilliant green and blue; each letter, of a rather coarse print, had a brilliant border of green; no other color was seen at this time; the white spaces between the print-lines of a page of the *Archiv für Ophthalmologie* were of a bright green, not at all yellowish.

Once only there were three colors present, all very brilliant and of fundamental tone. The usual patches of red and green were separated from each other by a band of even width of intense blue.

As some irregularity of accommodation has seemed to me to be among the possible causes of the phenomenon, I have frequently prepared for a nap by arranging in my field of view a white surface with darker objects raised two millimetres above it, and by the side of it a dark surface with white elevations. But, upon all such occasions, if I have fallen asleep at all, it has been without catching the phenomenon. It is possible that mere fatigue—general fatigue, of course, not visual fatigue—is the sufficient explanation, as suggested by Mr. Havelock Ellis in cases of neurasthenia and of mescal intoxication. It is also possible that, in the colors which are of normal occurrence, the violet is due to some reconstruction of the visual purple, and that the green is the green of contrast. I am familiar with the color of the visual purple when seen subjectively, for I have no trouble in getting at any time the normal erythropsia described lately by Dr. Ernst Fuchs. (See this REVIEW, Vol. IV., p. 221.) Some years ago, after walking over a bright pavement in a glaring sunlight, with one eye covered, in preparation for an experiment, I found that I could get this effect very brilliantly, and that it lasted for a long time —much longer than four minutes. The fact that it was only in one eye, and that the sensation of the other eye served to prevent the rapid fading out which occurs when any sensation is widespread and continuous, made the circumstances peculiarly favorable.

I confirm Fuchs' observation that the color does not extend quite so far as the field of vision, and that it is wanting at the centre. The color-tone of this normal erythropsia is quite the same as that of the violet on going to sleep. The other colors have, of course, no connection with this, but they belong, I believe, to a much deeper degree of sleep. I have frequently experienced the green vision (as well as the erythropsia) upon the first instant of waking up in the morning. This was explained by its discoverer as being due to the green fibres awakening a little sooner than the other two kinds—those both awak-

ening together, apparently! A recent writer in the *Wissenschaftliche Rundschau* believes it to be the contrast effect of the red caused by the blood vessels of the eye, to which a sensitiveness persists for a few moments; but if this were the correct explanation it would seem that the red ought also to be perceived, which is not the case.

I should be interested to know if any one else gets these colors upon the on-coming of sleep.

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PROFESSOR WUNDT'S 'UEBER NAIVEN UND KRITISCHEN REALISMUS.'[1]

In this article Professor Wundt handles with marked critical insight and thoroughness, some of the crucial problems which arise in the so called Immanental Philosophy of such men as Schuppe, Schubert-Soldern and Rhemke.

The burden of his criticism is that the logic of the system brings it into immediate conflict with the basal principles of Natural and Psychological Science as well as with their accredited results.

I. In denying any transcendence of the object the Immanental Philosophy runs counter to the naïve consciousness as well as the scientific critical reconstruction of the same. Believing with all other theories of knowledge in touch with the realistic tone of the time, that we must retrace the steps of original naïve knowledge undisturbed by reflection, except in so far as needed to correct errors, the Immanental Philosophy sets about the task of correction. This is, however, nothing less than an attempt to sweep away the whole 'absurd' notion of a transcendental object. It is concluded that if a transcendence of all experience is inconceivable, it is likewise impossible to conceive of a transcendence of consciousness, and so all reality is reduced to conscious content, as immanent in the subject. This reconstruction of naïve thought, since it takes away its fundamental concept, is impossible either for it or for the scientific reconstruction of the same. Natural Science recognizes all elements as objective reality which, without contradictions among themselves, remain after all abstraction of subjective perception. She rests her whole claim to objectivity upon the possibility of abstracting from the subject, and goes out from the principles: "*Jeden Inhalt der naïven Erfahrung so*

[1] An abstract of Professor Wundt's article in *Philosophische Studien*, XII., XIII., 1896, 1897.

lange als gegeben anzuerkennen als er nicht, durch nachweisbare Widersprüche, zu denen dies führt, als ein blosser Schein nachgewiesen sei." Now the point that Professor Wundt makes, and very properly too, is that this criterion of objectivity is part of the logic of Natural Science, and, since scientific processes are but a refinement of common naïve knowledge (with this advantage: that, by a long development through experience and reflection subjective elements have been eliminated, and a settled logic of scientific thought attained) scientific criteria of objectivity are final for any theory of knowledge. The Immanental Philosophy, in denying the concept of transcendent objectivity, in reducing all reality to conscious content, runs up against the logic of the sciences.

II. But the believer in the idea that *esse=percipi* has found in more recent times a new criterion which may be substituted for the scientific. He argues that the latter, demanding merely a contradictionless whole of experience, is in reality no criterion of the content of truth, but is purely formal and negative, and leaves the problem of truth unsettled except in so far as, by an endless series of experiments and abstractions, a relatively contradictionless whole of experience is attained. Therefore an *a priori* criterion is substituted—it is the test of the 'gattungsmässige' of the social consciousness, of social categories. What is socially experienced is true as over against the subjective opinions and errors of the individual. The problem is accordingly resolved into a conflict of criteria; and for the social criterion Wundt has nothing but ridicule. How, he asks, is the logical argument that the individual ego presupposes social consciousness a possibility without the very assumption of the existence of external objects in which the other consciousness belongs? Otherwise it is a mere abstraction. As for the empirical worth of the criteria of the common social consciousness, the simplest optical illusion suffices to show its uselessness, and the entire history of scientific method, splendidly illustrated in the Copernican System, shows that the real criterion of knowledge is the perception of the individual object, corrected by experiment.

III. The real difficulty comes, however, in the application of this criterion to the actual content of consciousness. What are the 'gattungsmassige,' the common elements of the social consciousness, when the individual ego is abstracted. First of all, *sensations* in space and time. Since they are given immediately in the naïve consciousness, and are common property of the *Socius*, they must be accepted as objectively real, and the question of their origin is not a proper problem for a theory of knowledge. Whether all sensations, or simply

those of the higher sense of sight, are to be taken as objective, the Immanentalists have not yet settled among themselves. But this is immaterial for the principle and the suggestion of Schubert-Soldern, that all sensations may be reduced to one ground sensation, the differences being merely qualitative expressions of relations which Natural Science seeks to express by means of its transcendental atoms, is a thoroughly logical solution of this uncertainty.

To Professor Wundt's thinking such a doctrine means nothing less than a denial of centuries of accredited work on the part of science, and, secondly, an utter confusion of the boundaries between Natural Science and Psychology.

Since Galileo, the great presupposition of modern science has been the subjectivity of sensations—and on the ability to abstract from the same, and upon their reference to external moving bodies, as their source, rests modern mechanics and molecular physics. The doctrine which maintains the objectivity of sensation runs counter to all this and the suggestion that all sensations might be reducible to a fundamental one would have as its logical outcome a rejuvenation of the Aristotelian color theory, which reduced all light phenomena to two ground sensations, light and dark. With modern optics Wundt holds it is entirely incompatible.

But the difficulties to which it gives rise in the determination of the boundaries between Natural Science and Psychology are alone sufficient to show the untenability of the doctrine of the objectivity of sensation. If sensations are objective, then they are the data of objective Natural Science. On the other hand, Psychology can have to do only with the 'non-gattungsmässige,' or individual elements of consciousness, which leaves no basis for general laws. The philosophers of this school, accordingly find in the reproductive processes of memory and imagination the sphere of Psychology, or otherwise distinguish it as the object of individual introspection, while the objects of Natural Science are the primary sensations of the many. But the simple facts of Psychology make impossible such a distinction. Between sense perceptions and the reproductive processes there is no distinct line, as Hume conceived in his impossible theory of strong and weak impressions. A pure sensation is an abstraction, sensation is not known apart from the reproductive processes. Thus, if sensations be the data of Natural Science, the latter must logically be subsumed under Psychology. This is however equally distasteful to both sciences. Natural Science cannot allow of such a subjectification of its problem, nor can Psychology admit this forma-

listic metaphysical determination of her field of work. The abstract concept of the individual is powerless to give her material and, without the investigation of the rise of sensations, it is impossible for her to understand the reproductive processes based upon them. The whole difficulty lies in the false presupposition that the two sciences deal with different kinds of content, a deplorable modern putting of the Kantian distinction of 'inner' and 'outer' sense. In fact, both deal with the same content, for there is only one object, but each approaches it from a different standpoint. Natural Science has found it necessary to abstract from all subjective elements, *including sensation;* it is just this subjective side, however, that is the peculiar sphere of Psychology, the whole immediate, subjective side of reality. If the ideals and methods of the sciences are understood there is perfect harmony. It is only where these are ignored, and *a priori* theories of knowledge, developed by uncontrolable reflection upon naïve thought, are substituted, that conflict arises.

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PSYCHOLOGICAL LITERATURE.

Philosophy of Knowledge: An Inquiry Into the Nature, Limits and Validity of Human Cognitive Faculty. GEORGE TRUMBULL LADD. New York, Charles Scribner's Sons, 1897. Pp. xv +609.

The stately succession of Professor Ladd's treatises in philosophical science, consisting of 'The Elements of Physiological Psychology,' 'Psychology, Descriptive and Explanatory,' 'The Philosophy of Mind,' is now carried forward by 'The Philosophy of Knowledge,' upon which at least one further installment, A Philosophy of Being, is, we are told, to follow. The present volume contains an elaborate treatment of the chief problems of the theory of knowledge. Not at first sight nor in its arrangement, but after some slight scrutiny, the interdependent structure and fine convergence of the argument appear. It is independent, if not original; and it is presented in a style which, if diffuse, elusive and sometimes pedantic, is of a remarkable continuity, an almost consistent academic elegance and at times an outspoken vigor.

Dr. Ladd's philosophy of knowledge centres, one may say, in the thought of the self-conscious self. In the immediately revealed nature and the express deliverance of the self we have the key to the problems of perception, of judgment, of memory, of reasoning, of the nature of the cause, of the nature of the object, of the nature of the universe. The ontological doctrines are in the present work, of course, only adumbrated.

The theory of the self, which had previously been set forth in 'The Philosophy of Mind,' is of marked interest. There had been those who held to a 'thinking substance,' a substratum of mind, or an indefinable 'subject.' Transcendentalists (most of them) and cruder spiritualists agreed in maintaining that there is a single agent or principle (neither party would accept the terms of the other) which knows in all knowledge, feels in all feeling, and wills in all volition, but which is not itself of the content of consciousness nor appears in an act of objective cognition amongst the beings known. Consciousness 'inheres in it,' or 'implies it,' or 'involves it as a condition;' it is the eye which, well as it may be aware that its own existence is necessary

to seeing, is not amongst the objects of its own vision. On the other hand was the proposed psychological analysis of the self into elements of the conscious content, the doctrine that it is no undecomposable entity or ultimate 'principle,' but in one sense the total group of presentations, in another a distinguished part of that group. The former of these doctrines Dr. Ladd rejects with energy as adding to consciousness an unmeaning encumbrance, the latter he apparently passes by as withholding from consciousness its most essential feature. It is a striking circumstance that his own theory takes elements in some sense from both. The self is, indeed, a fact known by and in consciousness, it is *the* fact so known, being the fact and entity of consciousness itself. It is not a thinking substance 'beneath,' and hence outside of consciousness, nor is it a complex group of the particular facts of consciousness. Rather is it a thinking substance within consciousness, known for what it is, a single and active being which exists only in being conscious. The view is not (or not merely) that ontological principles oblige us to assume a substance or unit-being for mind, but that the mind directly announces itself such a unit-being. We have here, in modern form and the explicitness that a sense of past controversy gives, a view notably like that of Descartes. The soul is one substance, but its whole nature is *cogitatio*. And, Dr. Ladd further asserts, 'states of consciousness,' psychoses, are not existences, but phases or acts of an existence. Lastly, the continuity or personal identity of a self in time consists in nothing else than its ability to refer to its own past by the act of recognitive memory and to build up in its successive states an orderly life.

The self-knowledge of the self in a single moment is the perfect type of complete knowledge; and the part the self plays in knowledge of whatever kind is all-important. Knowledge professes by its very nature to be an affirmation of existence transcending itself. The assertion that we only know phenomena is absurdly false to the facts of cognition. The object in order to be object is inevitably recognized as 'not me,' as 'out of me,' as 'not my state of consciousness nor any man's state of consciousness,' as 'extra-mental,' as 'transcendent;' iteration could hardly go further as to the 'realistic' import of cognitive states. But this is to say that the object is explicitly recognized as not *self*, that it is set over against a recognized self; that self-consciousness is thus an indispensable condition of objective consciousness. And there are also in every cognitive state ingredients of will and feeling which involve further references to self. The object is felt as an alien *other* contrasted with *me*, and it opposes me as '*that-*

which-will-not always as *I will.*' More than this, when we come to ask what manner of existence the 'transcendent' thing is, we can, in the last resort, draw our predicates from but one source; from the only being we immediately know, from ourselves. The continuous identity of an object is somehow conceived in terms of the identity of the subject. The 'causality' or action of an object is conceived in terms of our own conscious action. Our ordinary explanations have their origin and their justification in "*the primal and universal experience of man with the self, as consciously acting and having its activity resisted, while at the same time observing the simultaneous and succeeding changes which go on in the appearance of things.*" And "*if the cognized facts are deeds done by a self, with a consciously recognized end in view, then it is possible to explain to its very centre the 'reason' for the facts.*" Again: "*The grounds on which all acts of reasoning repose, so far as they can possibly be explored by an analysis of knowledge itself, are laid bare when we behold the nature of the self rewarding itself in the pursuit of some conscious good.* This is the final answer to the question: 'Why ?'"

When at last we set ourselves to realize the nature of the entire universe, our means of conception bear the same stamp. Our postulates imply (1) some sort of unitary Being for this really existent, (2) that this Being is Will, (3) that the differentiation of the activity of this Will, and the connection of the differentiated 'momenta'—the separate beings of the world—is teleological and rational. That is, it must be thought of 'after the analogy of the life of a self.' And in the concluding words of the book, knowledge is described as "the establishment of a relation between the Revealer, the Absolute Self, and the Self to whom the revelation comes."

In sketching thus in scantiest outline some noteworthy aspects of Dr. Ladd's work one is obliged to neglect many important discussions; such as his argument as to 'The Teleology of Knowledge,' his trenchant treatment of the 'antinomies,' his comments on the conception of invariable law, and his special form of the argument for a cosmic mind.

The species of 'Ideal-Realism' before us has a height and breadth of build and a harmony of form that places it amongst the more imposing styles of speculative architecture. This world of wills is interesting. Meanwhile its conception and proof contain, of course, save in slight details, nothing novel. There is, by the bye, a curious passage in the preface in which the author says of his volume: "It

asks and should receive the treatment due to a pioneer work." And again, remarking that his task would have been easier if he had had "more predecessors among modern writers on philosophy in English:" "So far as I am aware there are none from whom any help is to be derived." One does not know to just what species or degree of indulgence a pioneer-work in the nineteenth century upon some of the hoariest problems of philosophy lays claim; but one is, indeed, moved to make allowance for an author who has been unable to find stimulus or suggestion in the epistemological writing (for almost random example) of J. S. Mill, Mr. Bradley (a juxtaposition that only the former would have tolerated), Mr. Hodgson, Mr. Balfour (in his 'Philosophic Doubt,' for instance), or from any of twenty years' contributions to the periodical 'Mind.' The passage is mystifying, and one has a guilty sense of its being perhaps a needless touch of critical acerbity to refer to its existence. But whatever its exact meaning may be, the tendency is significant. The prime need of the day in our somewhat distracted science is to make discussion effective by bringing the opposing forces really to bear. As it is, two hostile theories will keep up their ceremonial duel for generations by simply firing into the air, which is thus filled with smoke and lurid flashes while their own vitals remain unharmed. A little marksmanship and economy of powder, and something may come out of the day; loud reports and beclouded fulgurations, pistol- or artillery-practice on whatever scale, are in themselves a dubious end. We shall do ill to forget that scientific labor will advance by growing genuinely social; that it demands a keen sense of what is already done and what needed. The spirit of lucid controversy is a fine effluence of civilization, and its effort to grasp extant ideas with delicate justice, and with precision of strength to break apart or rivet them closer, is almost the worthiest discipline of the intelligence. We can hardly look for a signal exhibition of it from one who stands in the thick-trodden market-place with the unshaken conviction that he is a pioneer.

And, in fact, on certain long-controverted topics, Dr. Ladd shows no such fine sense of his own or his adversaries' position as would enable him to carry us an inch nearer to 'a consensus of the competent.' This is markedly true of his theory of the realistic import of cognitive consciousness. The phenomenist might simply deny the alleged trans-subjective intention, the implication of the 'transcendent' in knowledge—as the present writer at all events does deny it—might declare that there is no such psychological fact; and Dr. Ladd could have nothing for it but asseverations. But not by the 'assertory

method' on either side is inquiry furthered. Rather by something more analytic. And upon analysis it turns out that no such 'extra-mental reference' is psychologically possible. In order that our consciousness should affirm that something does not belong to it, it must have a generic conception of itself, an accurate universal idea of what 'belonging to consciousness' means. Now it is obvious that those familiar authorities, the child and the rustic, not to come nearer home, have no such idea. Such an accurate idea—and a testimony of consciousness based on inaccurate ideas of the situation would hardly be cited as evidence—is a complex product of philosophic analysis. Epistemological realists (notably Mr. Spencer and Professor A. Seth) have sometimes spoken as if idealists supposed that what we first know in perception is that we have a sensation, a mental phenomenon; a view which they very easily refute by showing that 'sensation' or 'mental phenomenon' is a later conception than object. But if they add (what they mean) 'extra-mental object,' if they declare that what we are first conscious of is that here is an object external to consciousness, they have confuted themselves in advance. To judge 'Extra-mental!' is to have a conception of the mental, which they have just pronounced at this stage impossible. The inference is that the first stage is to have a sense of the presence of an object—*to have an object*—without classifying it either as mental or as extra-mental. It is to the subsequent reflection of the metaphysician that grounds appear for terming it mental.

To this Dr. Ladd might, perchance, reply that no complex conception is needed but only the *perception* or immediate consciousness of self. To judge a thing in cognition as 'not-self' is to contrast it not with an abstractly conceived but a directly felt self. The answer is (even granting our author's theory of the *ego* and our knowledge of it) that such a concrete perception of self is not sufficient to yield the clear deliverance of consciousness on which he relies. It is not enough to deny that the object is the self; of course it is not the self. The question is whether it is not a content-fragment of the self's consciousness. And it is not enough to deny that it is *this* content-fragment or *that* content-fragment; of course there are content-fragments which it is not. The question is whether it is a content-fragment at all. To deny that is to employ the generic conception.

But, perhaps, Dr. Ladd means simply to reaffirm the old doctrine that a certain psychosis, namely, a cognition, concretely distinguishes its object from itself, pronouncing the former external to itself. In that case, forbearing remark on the logical atrocity here perpetrated,

one might rest content with the old appeal to introspection. Perception is not thus double; we do not have in view an object *plus* a professed percept; we have in view only an object; and the whole psychological phenomenon is what we subsequently class as a percept. It may, however, further be said that distinction is a mental process involving two mental terms and that to say that one mental term is not another is true, but does not assert the existence of anything extra-mental. Is it suggested that the discrimination is between one term and the thing expressed by the other? The suggestion assumes all over again and without analysis the possibility of that 'expression of the transcendent,' that 'trans-subjective reference,' which is the very matter of dispute.

Lastly, if our author should maintain that the 'trans-subjective reference' is precisely an unanalyzable and ultimate fact, a mysterious 'meaning of the mind' or 'cognitive property of thoughts' which may be felt but not understood, one may reply in Mr. Meredith's words: "Many people are mystics until they have written out a fair copy of their meaning."

For not only does psychology find no terms in which such a 'reference' could be consummated, but logic exposes it as a self-contradiction. One existence, a thought (or if our author will, a thinking self) is to designate another existence, an extra-mental object. How is it to do so? It may resemble such an object (if the object be of a psychic nature the idealist need not deny that it may exist) or contain something resembling it. But no, it must also contain indications of the numerical identity of the object as different from its own. Now this is in the end unmeaning. One existence may resemble in quality another and thus to a person informed (as consistently with phenomenism one in some cases may be) that the former is to serve as a sign or representative of the latter, convey its character. But it cannot (so to speak) resemble another in its numerical identity and thus by its own being convey the existence of something else. One is familiar, of course, with the common mode of speech as to the manner of this conveyance, about a cognition containing the object ideally without containing its reality. It must be remembered, however, that ideas and cognitions are realities too and what we are discussing is the relation between two realities. An idea may actually operate so as to guide our conduct towards something not itself. But it cannot, without sharp self-contradiction, be said to contain what is non-identical with any or all of its parts. Until something is done to relieve this difficulty the theory of a trans-subjective reference stands discredited.

In the other great problem of epistemology, the ground of inductive generalization, and in further points of theory that cannot here be touched on, Dr. Ladd tries to improve the situation rather by firmer exposition than by deeper analysis. I cannot think that one who had fully mastered Hume's arguments as to cause and effect could imagine their force to be dissipated by interpreting the relation through 'the self's experience of its own action and suffering.' This is a door of escape that Hume expressly guarded. That form of sequence which we know as our own mental activity reveals virtue going out of the cause into the effect as little as any other sequence in experience; and it reveals the cause as sufficient condition of the effect no whit more than any other. To conceive material bodies acting as we feel ourselves to act—to conceive their action as a series of conscious states external to our own—is not to conceive them as extended masses at all. As such a philosophy is talking about something else than the plain man's matter, it cannot hope to have epitomized the plain man's notion of material cause. Cause cannot have meant to the multitude of men what such a philosophy means by it.

As already said, the root of Dr. Ladd's theory of knowledge lies in his theory of the nature of mind; and of that there is small room to speak here. If the unit-self is a fact of consciousness and exists only in consciousness and comprises the whole being of consciousness; then it simply *is* consciousness, called a unit-being: and whether justly so called depends upon the meaning of unit. Consciousness, as Dr. Ladd amply testifies, has multiplicity; it has unity, or rather (for that term, through varied usage, has lost the edge of its meaning) it has conjunction of elements into a group or whole, in that there is an ultimate relation of jointness between (for instance) my taste and my hearing at this moment, which does not subsist between my taste and your hearing; and in that there is a relation of continuous change (not ultimate, but analyzable in a complex formula) between the total present consciousness called mine, and any total past consciousness called mine. When, however, Dr. Ladd incidentally drops that a so-called 'state of consciousness'—the total consciousness of a moment—is not an existence, but the mere state or the mere act of an existence, he is led away by the scholastic terms he has used in rendering the deliverance of consciousness about itself into an ontological inference that flatly gives the lie to that deliverance. Consciousness in its totality at this moment—what is called in the old loose terms 'my present state of consciousness'—is identical with the unit-self at this moment in what ought in consistency to be Dr. Ladd's sense of the word.

In this fragmentary comment, which can pretend to no kind of complete justice to a work so comprehensive and mature, there has been much mere assertion. So far as this has failed to suggest the analyses which could not in full be here performed, it has been of course quite futile.

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PSYCHICAL RESEARCH AND PATHOLOGY.

Involuntary Whispering Considered in Relation to Thought-Transference. HENRY SIDGWICK. Proceedings of S. P. R., XII., 298–318. December, 1896.

Messrs. Lehmann and Hansen, it will be remembered (PSYCH. REV. Vol. III., p. 98), sought to prove that a certain series of experiments in thought-transference, by Professor and Mrs. Sidgwick, were explicable because the agent's inward articulation of the numbers guessed was probably heard hyperæsthetically by the hynotized percipients. Repeating the experiments so that the percipient could actually hear the agent's suppressed whispering, they found that not only the successes, but also the mistakes resembled those in the Sidgwick series, and from such like effects they think that we ought to infer like causes.

Their paper, the carefulness of which is a refreshing exception to most criticism of the Psychical Research Work, is reviewed by Professor Sidgwick, who concludes that their experiments do not show positive evidence for whispering as the source of the English results. Much of his reply is too minute for reproduction. The most telling point he makes is an empirical one. Happening to have the record of an old series of pure chance-guesses at numbers, made with the agent and percipient in separate closed rooms, he compares this with the guesses of the Danish series. Of course, the number of successes differ widely in the two series, but the errors run even more closely parallel than they do when the Danish whispering series and the English 'thought-transference' series are compared. As such an amount of similarity in error with the whispered series is obviously fortuitous in this case, so it may be fortuitous in the thought-tranference case. Professor Sidgwick would partly explain the degree of similarity found (which is but slight[1]) by an unconscious preference for

[1] The Danish authors made only 500 experiments, obviously too small a number for safe conclusions. The better to frame critical opinion, I

certain numbers in the guesses of both sets of percipients. If, for example, both tended frequently to guess 'five,' five as a frequent error would occur in both series, and make them in so far forth agree.[1]

Sidgwick, although admitting that whispering may possibly have been a cause of successful guessing when agent and percipient were in the same room, thus denies that Professor Lehmann has proved the point. And he absolutely denies Lehmann's explanation where the agent and successful percipient were separated by closed doors. Passing to a general discussion of the subject, especially so far as drawings were the things guessed, he gives a *resumé*, in brief, of the whole body of evidence which many readers will find a convenient summary to refer to.

I Fenomeni Telepatice e le Allucinazione Veridiche; Osservazione Critiche Sul Neomisticismo Psicologico. ENRICO MORSELLI. Firenze, Landi, 1897. Pp. 58.

A courteously written plea against accepting the recently published evidence for thought-transference and veridical hallucination. The

have myself collected a series of upwards of 1,000 guesses at bi-digital numbers whispered with closed lips by the agent. Following Lehmann's method, and comparing the four most frequent erroneous guesses at each digit of the numbers whispered with the four most frequent errors made in divining the same digits in the English thought-transference series, I find (taking the digits from 1 to 9) that 20 of the erroneous digits are common to the two series. But I find that if one compares the four *least* frequent erroneous guesses in my whispered series with the *most* frequent corresponding ones in the thought-transference series, one gets 15, no great difference. Taking the one most frequent error of substitution for each digit in my series, I find but 2 agreements with the thought-transference series, and 2 with the Sidgwick series of pure guesses. Plotting the frequency of the various errors in the several series as curves shows so great a discrepancy between my whispered series and the Danish one that it becomes obvious that the series are too short to serve as proper terms of comparison with the thought-transference series. Moreover, the curves of my series and those of the thought-transference series show at special points variations from each other so great, when compared with the absolute figures which they represent, that the same conclusion is again obvious. Both the agreements and the disagreements are thus probably accidental. I, myself, agree then entirely with Professor Sidgwick that Professor Lehmann has failed to prove his particular hypothesis of whispering as the cause of the 'thought-transference' results; and I am pleased to notice that Mr. Parish, in the work noticed below (Hallucinations and Illusions, p. 320, note), also considers Professor Sidgwick 'perfectly justified in his contention.'

[1] In my own series, the tendency to run on favorite numbers in guessing was a well marked phenomenon, to eliminate the effects of which many thousands of guesses would be required.

familiar methodological generalities about what should constitute satisfactory scientific evidence for such phenomena are laid down at excessive length, but the author gets in some short-range work in criticizing the evidential defects of several narratives published as good ones by the French, Italian and English psychical researchers. A curious prejudice runs through his pages that no evidence for supernormal cognition can be drawn from cases of persons of neuropathic constitutions, or from those in whom there have been multiple experiences of the sort. He even thinks that he discredits veridical apparitions by saying that the majority of them seem to have occurred in 'English misses' at the change of life. Can he be so sure in advance that neuropathic constitution, or even the 'menopause,' might not be predisposing conditions for telepathic susceptibility, if such a thing should, in point of fact, exist? And, as for persons with multiple experiences, they would seem *a priori* to be just those from whom evidence might be best obtained. In point of fact they are so—one subject of 'psychic temperament' being worth many with single experiences. Professor Morselli, at the close of his pamphlet, gives a list of conditions which he seems to regard as alternatives to telepathy—no case should be counted as telepathic if it be possible to conceive it "under one or another of the following psycho-physical explanations; simple suggestion, auto-suggestion, individual and collective credulity, psycho-physical automatism, hypnoid or sub-conscious conditions, sensorial illusion, psychical illusion, *e. g.*, from accidental coincidence, provoked hallucination, especially with *point de repère*, unconscious perception, emotion or movement, involuntary expression of one's own thought, doubling of personality, dream or hypnagogic hallucinations, illusions of memory, after-images or retarded sensations, sensations induced by imperceptible or unappreciated physical agents (heat, electricity, magnetism, light), conditions of ecstacy (monoideism), hysteria, epilepsy and epileptoid, cataleptic, or somnambulic states, with loss or obscuration of consciousness, lucid forms of insanity, especially with hallucinatory fixed ideas, psychic mimicry and imitative of psychosis, or collective hallucination, intense emotional conditions with their effects, transient states of cerebral intoxication, whether endogenous or exogenous * * * ." Once more, one is tempted to ask why must all these things be *alternatives* to supernormal cognition? Why, if it exist at all, may it not co-exist with some of them? Why, indeed, may not some of them be its most predisposing conditions? Again, in point of fact, if there be supernormal cognition, it looks as if this were the case with it.

It is a pleasure to turn from the generalities and abstractions of the learned Genoese professor to the criticism at closer quarters of the next author on our list.

Zur Kritik des telepathischen Beweismaterials. EDMUND PARISH. Leipzig, Barth, 1897. 8°. Pp. 48.

Hallucinations and Illusions, a Study of the Fallacies of Perception. EDMUND PARISH. London, Walter Scott; New York, Charles Scribner's Sons. 1897. 12°. Pp. 390.

The English version of Mr. Parish's book, already reviewed in its German shape in Vol. II., p. 65 of this REVIEW, is greatly improved and brought up to date. The author incorporates in it much of the criticism contained in the lecture '*Zur Kritik*,' etc. He was collector for Germany of the Census of Hallucinations reviewed there and in the present lecture he criticizes the Sidgwick report. Although he gives the authors credit in the handsomest terms for the quality of their work, he nevertheless thinks that their conclusion—that apparitions on the day of death are far too frequent to be ascribed to chance—will not hold good. His chief reasons are as follows: First, they have believed the reported amount of coincidence between the apparition and the event to be greater than facts warrant. He gives cases to show how a figure, not recognized when seen, may be described, when news of a death is later received, as the figure of the person dead. This error, which he calls *Erinnerungs-adaptation*, he believes to be very frequent in the narratives. Secondly, he doubts whether most of the hallucinations which figure as veridical are *waking* hallucinations at all, believing them to be more probably dreams or hypnagogic visions. But if dreams are to slip in and get counted, the numerical statistical argument, he says, is entirely upset; for dreams are such frequent occurrences that coincidences between them and distant events must be frequent in proportion. And that the so-called waking hallucinations *were* mostly dreams, he proves in detail by analyzing the 26 cases which the English report prints as 'best accredited.' Most of them actually occurred at night, when the percipient was in bed or sitting up watching, or else in some other situation where a nap might naturally have occurred unawares.

This latter seems to me by far the strongest objection yet made to the Sidgwick report. In my own review of the Sidgwick report (*supra*, Vol. II., p. 74, note), I admitted this to be its weakest point.

But another objection of Herr Parish's, and the one which he himself considers his weightiest, seems to me to have very little

weight indeed. He shows, by three examples, through what subconscious links of association, granting the hallucinatory tendency to be there, the ensuing hallucination may have its subject-matter determined, and then says: Not till *the possibility of all such associative links is excluded*, are we entitled to invoke an hypothetic agency like 'telepathic impact' as the cause of the hallucinatory content. But one does not see how this should effect the statistical argument, unless associative links are in themselves more likely than unassigned organic or other causes to produce visions *coincidental with deaths*. If the mental associations of the percipient belong to a cycle of events disconnected with the cycle concerned in the distant person's death, it remains as improbable as ever that the several outcomes of the two cycles coincident in content should also coincide so often in *date*. That they actually do so shows, according to Mr. Parish, a methodical flaw in the Sidgwick report. Its authors accept as an empirical fact (with a slight correction for oblivion) the measure of frequency given by the Census for visions of recognized persons, and then proceed to cipher out the improbability that any one such vision will occur by accident on the day when its object dies. But they ought rather, says their German critic, to have ciphered out, from the number of *such coincidences* as an empirical fact *what the real frequency*, as distinguished from the recollected and reported frequency, of the visions must actually have been. This would give (as I apply his reasoning) the figure of 35 hallucinations at least, of the species immediately discussed, to each adult in the community, and 60 times that number, or over 2,000 miscellaneous hallucinations of all kinds to each head of population,[1] most of which we must suppose to be forgotten immediately, if the reasoning is to be seriously applied to facts. Mr. Parish, of course, would not so apply it, for the result is absurd and incredible. He only makes a logical nut of it for the other side to crack, disbelieving himself that the returns of the Census have any definite numerical value at all. In this contemptuous estimate I cannot possibly agree. W. J.

[1] The computation is this: By the English figures 17,000 persons yielded 32 death-visions, each of which had only 1 pure chance in 19,000 of occurring when it did. To produce the 32 happy chances there must, therefore, have been 19,000 × 32 such visions in the whole 17,000 persons, or 19,000 × 32 ÷ 17,000 = 35.7 such visions in each one of the 17,000. But, since the 32 death-visions were extracted from 1,942 hallucinations of all kinds experienced by the 17,000 answers of the Census question, each answer must have had a number of hallucinations of all kinds as much greater than 35 as 1,942 is greater than 32, which would give him approximately 2,000 hallucinations, not one of which in 9 cases out of 10 he would have remembered, for roughly 9-tenths of those questioned in the Census replied 'No.'

Lo Studio Dell' Attenzione Conativa, Ricerche Sperimentali. SANCTE DE SANCTIS. Atti della Società Romana di Antropologia· Vol. IV., Fascicolo II. Pp. 19.

Experiments on the changes in the extent of the field of vision when the attention was distracted, first, by auditory appeals, or painful stimuli to the skin; second, by the task of counting the number of details in a circular diagram presented at the centre of the field. Two normal subjects (one more cultivated and intelligent than the other), one melancholic, and one 'hallucinated' (paranoiac?) subject were tested by the perimeter. The results showed contraction of the field in all cases. The contraction was only moderate under the first kind of distraction. Under the second kind it was considerable; the paranoiac suffering in both cases the greater loss.

W. J.

Collezionismo e Impulsi Collezionistici. SANCTE DE SANCTIS. Bulletino Della Società Lancisiana Degli Ospedali di Roma, Anno XVII., fasc. I. Roma, Tipografia Innocenzo Artero, 1897.

A careful discussion of the definition of 'Collectomania' or 'morbid collectionism,' on the basis of the case of a woman of 63, in poor health, with depression of spirits, who took to collecting and secreting pieces of bread, hair, bones, and refuse of all sorts. Wherever she saw any such object lying she was compelled to pick it up and add it to the collection. She complained of the symptoms as a sort of insanity, and said they were relieved when she drank wine; and this induced the doctors, suspecting alcoholism, to proscribe wine altogether, when the symptoms disappeared. When pressed for her motives, she at last reluctantly said that she thought the neighbors might use the objects in question for casting spells upon her.

De Sanctis finds the absence of consciousness of morbidness, and of reason for the acts, not essential to the definition of morbid collecting mania. The coercive impulse, he thinks, is primary, explanation secondary; and the sense of foolishness which may in any case exist on reflection between active fits, may be more or less actively or constantly present, according to the freshness of the case, or the intelligence of the subject.

W. J.

Sulle Cosidette Allucinazioni Antagonistiche. SANCTE DE SANCTIS e MARIA MONTESSORI. Roma, Società Editrice Dante Alghieri, 1897. Pp. 17.

This article contains a detailed description of a number of clinical cases in the University of Rome which presented the phenomena of

antagonistic hallucination. The various symptoms observed seemed to admit of a classification somewhat as follows: 1. As to whether the opposed hallucinations appeared as simultaneous or successive. 2. As to the manner in which the hallucination is received by the sense organ, as in hearing whether the one hallucination is heard in one ear and the opposed in the other, or whether the two are heard promiscuously in both ears. 3. As to whether the hallucinations are homonymous or heteronymous; that is, whether perceived in one and the same sphere of sensation or in different; for instance, the two hallucinations may both be auditory, or one auditory and the other visual. 4. As to whether the antagonism observed manifests a logical contradiction and is therefore absolute, or whether the antagonism is one which is rather personal and relative. 5. As to whether the hallucinations are of a physical or psychical nature. 6. As to whether the hallucinations are episodical or accidental on the one hand, or on the other enduring and systematic.

The explanation of the antagonistic hallucinations as given by the authors of the pamphlet is that of a tendency to association by contrast which tendency in pathological cases is abnormally exaggerated, giving rise to the opposed hallucinations as observed.

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SOCIAL PSYCHOLOGY.

The Principles of Sociology. FRANKLIN H. GIDDINGS. New York, The Macmillan Company. 1896. Pp. xvi+476. 3d Edition. 1897.

The Theory of Socialization. Same author and publishers. 1897. Pp. xiv+47.

The Genesis of Social Interests. J. MARK BALDWIN. The Monist, April, 1897.

The psychologist can scarcely read Professor Giddings' 'Sociology' and its accompanying syllabus which puts into connected form the theoretical principles and psychological presuppositions of the larger work, without feeling convinced that the day of psychology has only begun to dawn. Unless all signs fail, the study of sociology is to take its place not merely in the graduate schools of universities, but in the under-graduate work of the colleges, side by side with economics and politics. Professor Giddings book, by its more rigorous effort to define the province of the science, and to determine the fundamental unit of

explanation, will be the most potent instrument which has yet appeared in making possible such a recognition in educational curricula of the investigations which reflect the actual interests of our day. But if sociology, as in these works, is defined as a psychological science, or even as a branch of psychology, it is evident that this must mean a highly increased interest in psychology and a new demand upon the psychologist. Just as the eighteenth century's interest in the individual's moral life called out the psychology of its time, just as the nineteenth century's interest in natural science has created the experimental psychology of to-day, so the growing social interest promises to evoke a social psychology, which in my judgment has a broader field than any of its predecessors.

The delimination of sociology from psychology is to be effected, according to the author, "by restricting psychology to a study of the phenomena of the individual mind and by assigning to sociology the investigation of the more special and complex phenomena of minds in association with one another." "Psychology is the science of the association of ideas. Sociology is the science of the association of minds" (Princ. Soc., pp. 241). The impossibility of such a delimitation has been pointed out in previous notices of 'Social Psychology' in this REVIEW. Just as there are no individual 'ideas' which can be studied, in isolation, so there is no individual mind which can be studied and comprehended apart from its relations to others; and just as psychology has learned that no 'association' of such ideas could make up a mind, so sociology must learn that no association of individual minds—if they were really individuals which could be dealt with as such—can make a society. A true analysis of the social condition will not give an 'individual' as its unit for synthesis.

In the syllabus the formulation is somewhat different. The 'four great processes which make up the practical activities of life' are there stated to be: (1) 'Getting used to the world by attempting to obtain the utmost knowledge and feeling from external things,' which is the process of 'appreciation.' (2) 'Adapting the external world to ourselves,' 'utilization.' (3) 'Adapting ourselves to the external world,' 'characterization.' (4) 'Adapting ourselves to one another,' 'socialization.' These four processes are studied respectively, by psychology, economics, ethics and sociology. It is somewhat startling to be told that ethics deals rather with our relations to the external world than with our relations to each other, and the author himself states that psychology deals with more than the process of appreciation, so that it is to be feared that the fascinating symmetry of

this latter scheme will not avail to make it practically workable. Moreover, to the psychologist the definition of his own subject as the 'science of the association of ideas' has an anachronistic sound, which prepares him to be suspicious of the presuppositions involved in a 'science of the association of minds.'

What these presuppositions are appears in the author's fundamental principle of 'consciousness of kind.' This is attained as follows: Socialization requires some degree of similarity, of which the important modes are three: (1) Kinship. (2) Mental and moral similarity. (3) Potential likeness or capacity for assimilation. "Consciousness of kind is a state of consciousness in which any being, whether low or high in the scale of life, recognizes another conscious being as of like kind with itself" (Pr. Soc., p. 17). This is analyzed in the syllabus into a combination of (a) perception of resemblance, (b) sympathy and liking, and (c) a desire for recognition. It is declared to be the simplest or elementary social state of mind, and the chief socializing force. It modifies appetites and desire, 'impression' (the mental power of one over another) and imitation so that the individual motives become socializing forces.

The first query which suggests itself is as to whether we are not liable to assume an altogether too highly developed consciousness if we make a consciousness of kind co-extensive with society. It is, of course, true that a bee or dog or horse reacts in a peculiar way toward what we call his kind, but this is far from implying that there is any trace of such a process in the animal consciousness as a recognition of likeness. It is well known that smell is the organ mainly depended on by many of the sociable animals, and it seems far more likely that there is no comparison of the odor from the other, with that from the animal's own body, but merely a direct reaction upon a grateful stimulus. Nor does the attachment of kinship seem to me to be the result of any such comparative process. So far as it is found in higher animals, or even in simpler human conditions, it seems to be in the case of parent for offspring, a direct instinct, selected in the struggle for existence; in the case of offspring for parent, it doesn't exist, *i. e.*, any source of nourishment and entertainment is equally liked by the little animal or human infant, unless other circumstances, familiarity, etc., enter; in the case of brothers and sisters, it doesn't exist as the result of a perception of kinship or resemblance—any group of children brought up together from infancy develop as much affection as the average family of the same blood. In a word, the attachment is either a direct, not an indirect instinct in which there

is no comparison prior to attachment, or it is the result of processes much more intricate than the perception of resemblance, which appear clearly in the case of clan feeling. The theory, as applied in such simpler cases, implies that there is a consciousness of self as a definite, distinct individual and that others are compared and found to agree or disagree. This is an assumption which the psychologist will be slow to admit.

Is the difficulty met by the point of view of the syllabus, which regards 'consciousness of kind' not as a simple recognition that another is of like kind with the self, but as a complex state, involving the three elements named above, with the chief emphasis upon sympathy which is, indeed, often used as epitomizing the phrase? This will depend on what is understood by sympathy (Syllabus, 126). If it is conceived merely as 'imitation of emotions,' then, as Spinoza saw, it may mean emulation or conflict as well as compassion or sociability. If it is individualistic in its elements the combination will not be social. Sympathy, as a social force, must mean not a state in which A and B merely *have* like feelings, not merely a state in which B's feeling is caused by associations evoked by perceiving A's actions when under said feeling, but a state in which A and his feelings have really become a part of B's own interests, *i. e.*, of B's self, so that B is no longer a particular, exclusive self, who is B and B only, but is rather a self that includes A within it, in fact, a truly social self. Professor Giddings speaks of the case of two persons mentally giving and taking and thereby becoming alike. This is to stop just this side of insight into the essential factor in the social. The point is that they not merely become alike, but that the content of each personality is made to include something of the other; the self, while still individual, is not particular, but social.

This is, in fact, the fundamental inadequacy in Professor Gidding's psychology of the social self, that he treats it as the older psychology treated ideas, as ready-made, irreducible units, which could be associated, but not analyzed. If he had read and taken to heart James' chapter on the self, it could hardly have failed to suggest a different treatment, but Mr. Spencer seems to be the author's chief and almost sole authority in psychology.

It seems ungracious, however, to find fault with a sociologist for his psychology when one reflects how little psychologists have done in this field of the analysis of the social self since Adam Smith sketched the origin and growth of the moral sentiments. In fact, in view especially of recent French investigations, it might fairly be said that

sociology is at present doing more for psychology than psychology is doing for sociology. A work like this of Professor Giddings, which brings together such a mass of material, and makes so strenuous an effort toward its explanation on psychological principles, comes to the psychologist as a distinct challenge to a more adequate analysis of the social consciousness.

Professor Baldwin's article is a successful attempt to meet the challenge, not necessarily of this special work, but of current social and political theories. The concept of person is shown to be a complex content, in which the 'myself' is always merely one pole of a shifting field of other selves, and so is always defined in terms of others, just as others are defined in terms of it. In the case of a child in a family, the other pole is sometimes the parent, in which case the 'me' of the child is imitative, feeling itself to be the inferior pole; or, again, the other pole is the younger brother or sister when the 'me' becomes aggressive and exploits its superior power. The point is that it is absurd to call the child altruistic in the one case and selfish in the other, since its attitude is in both cases alike, the result of the other pole. The other pole, the *socius* or *alter*, is then fundamentally essential to the content of the developing consciousness of self, for the child not only thinks of the other, the *alter*, as his *socius*, but he thinks of himself as the other's *socius*. 'In short, *the real self is the social self, the socius.*' A child's 'self' will then normally include as part of its content, the family or group, his 'interests' reflect the interest of the group, and this identity of personal and family interests 'is responsible for the rise of the family, considered from an evolution point of view.'

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VISION.

I. *Weitere Beiträge zum Sehenlernen blindgeborenen und später mit Erfolg operierten Menschen, sowie zu dem gelegentlich vorkommenden Verlernen des Sehens bei jüngeren kindern, nebst psychologischen Bemerkungen bie totalen kongenitalen Amaurose.* W. Ubthoff. Zeitschr. f. psych. u. physiol. d. Sinn. XIV., 3, 197–241.

II. *Demonstration des Scheinerschen Versuches nebst Betrachtungen über das Zustandekommen von Raumvorstellungen.* Heine. Zeitschr. f. psych. u. physiol. d. Sinn. XIV., 4, 274–281.

Professor Ubthoff reports upon the development of visual ideas in three subjects. The first is an intelligent boy operated upon for congenital double cataract at the age of five. His mother declared that he had always been totally blind, but some tests seemed to indicate that he received vague sight impressions from large or moving objects, and that he could probably distinguish between large surfaces of red and green. The second (previously reported) is a feeble boy, operated upon at the age of seven for total, double congenital blindness, examined for some time then and again after a lapse of two and a half years. The third is a girl who became totally blind when four months old and received sight by operation at the age of three and a half years. The genetic theory of sight is upheld. The rate of development in vision differed surprisingly with the difference in the general intelligence of the subjects. The experience of the first subject in learning to recognize his own image in a mirror is specially valuable as a description of the struggle and fluctuation in the development of the consciousness of self. Colors were taught with difficulty. Color surfaces were at first only 'objects.' Indirect vision was much slower to develop than direct vision. In counting by sight the subjects moved their heads along the series instead of turning the eyes. The perception of form, size and distance was at first impossible and was learned through association with touch, as the author thinks.

The last section of the article is a report of interviews with a congenitally blind (microphthalmic) woman aged 32, with good power of introspection. She has no conception of light or darkness, but has marked preferences for colors. These are due entirely to associations, *e. g.*, she dislikes red because it is gaudy, something unbecoming to an unfortunate, and perhaps more so because as a child she was told that the flames of a certain fatal fire in the vicinity were intensely red. Her æsthetic conceptions are based rather upon intellectual and emotional grounds than upon sensory images. In remembering adults she constructs images on a large scale after the fashion of a child she has handled. She can form no conception of a picture. She commits to memory better when listening than when reading from the blind alphabet. She perceives the approach of large objects mainly by differences in 'air pressure.' She has medium power of estimating the distance of objects by sound and tactual space by movement. Tests with the æsthesiometer reveal no finer skin sensitiveness than the normal. This suggestive report upon observations reveals the need and possibility, and perhaps some method of experimental investigation on the subject.

Heine explains an excellent method of demonstrating Scheiner's experiment on a large scale. It is done by imitating the mechanism of the eye by arranging screens and refracting media so that all the conditions of the image are plainly set forth. By this means he demonstrates the condition of the emmetropic eye and reproduces the effects of myopia and hypermetropia. Using the same apparatus with two slits covered by differently colored glasses, he explains the physical and physiological conditions of the single image in binocular vision.

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PEDAGOGICAL.

Der Stundenplan. Ein Kapitel aus der Pädagogischen Psychologie und Physiologie. H. SCHILLER. Heft. I., Abh. d. Paed. etc. Berlin, Reuther u. Reichard, 1897. Pp. 65.

Although no psycho-physiological topic has received so much attention of recent years as that of fatigue, yet there are a number of reasons why the results of the experiments usually employed in these investigations are of a doubtful pedagogical value: (1) The tests are of an *unusual* nature, such as memorizing meaningless syllables, etc. (2) They are to the last degree monotonous, and, therefore, awaken no interest in the persons experimented upon. (3) They are protracted over unusually long periods of time, with no intermission or change. In a word, the conditions of the experiments do not correspond to the actual conditions of the school-room. Professor Ebbinghaus, of Breslau, has attempted to avoid this difficulty by testing the faculty with which children solve simple mathematical problems during the first ten minutes of each recitation, and he has tried similar experiments for the special purpose of testing the pupil's memory during different hours of the day. Dr. Griesbach, of Mühlhaus, has suggested that there exists a close connection between fatigue and the sensitiveness of the skin as tested by one's ability to distinguish two slightly removed points of a compass. This method of determining the amount of fatigue has proved to be the most satisfactory of all. The question of fatigue lies at the basis of an intelligent school programme.

The only complete restorative from fatigue is sleep. According to Axel Key, children between six and eleven, may, without hesitation, be allowed from ten to twelve hours daily, and even more; while youths of seventeen or eighteen should be allowed not less than nine

or ten hours. These figures should be borne in mind in fixing the hour for opening the school. The children should not be deprived of their sleep, nor, on the other hand, should the hour be fixed so late that they lose their freshness before school begins. No hour is suitable for all localities and under all circumstances, since the habits of the people vary. One can only say that in general residents of the large cities, except those of the manufacturing quarters, keep later hours than those of smaller towns and the country. With due allowance for local usages, Professor Schiller thinks the three lower classes should not assemble before nine o'clock, and he thinks there can be no doubt that seven o'clock, the usual hour in Germany during the summer semester, is too early.

More difficult is the question of a suitable closing hour and the distribution of pauses for recreation. The usual morning session in Germany lasts five hours, with a short recess after each hour. The object of the recess is complete recovery, if possible, from the fatigue of the preceding exercise, so that the amounts accomplished during the various periods may be nearly equal. In spite of numerous experiments, it is still impossible to determine exactly the length of the recesses necessary to this purpose. In the gymnasium at Giessen there is a ten minutes' recess after each hour except the second, when there is fifteen minutes, and each successive hour is shortened by about five minutes, the figures varying somewhat for the upper and lower classes. This system gives far better results than the old practice of allowing only one recess of fifteen or twenty minutes in the middle of the forenoon.

Dr. Wagner, of Darmstadt, has employed the method of determining fatigue suggested by Dr. Giesbach, with the following results: Fatigue increases with the greatest rapidity during the first hour, after which it increases but slowly. Mathematics, Latin and written exercises are specially fatiguing. Exercise in the gymnasium brings no recuperation, but often, and in proportion to its intensity, increases the fatigue. In accordance with these results, Professor Schiller proposes that the first hour be devoted to the most difficult subject, either Latin, mathematics or those subjects which require written exercises, preferably the last. These are difficult because they are more or less abstract and awaken little interest in the mind of the average child. They should be followed by subjects of more general interest, such as History, Geography, Religion or Drawing. The second hour is followed by a somewhat longer recess, after which the child is prepared for another difficult subject, preferably mathematics

and the foreign languages. The last hour should be devoted to the natural sciences, writing, singing, gymnastic drill, etc. When only two hours a week are devoted to a subject, it gives much better results if the exercises come on consecutive days, or even on consecutive hours.

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ÆSTHETICS.

1. *Gedanken zu einer Æsthetik auf entwickelungsgeschichtlicher Grundlage.* KONRAD LANGE. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, XIV., 3. May, 1897.
2. *Kritische Studien zur Æsthetik der Gegenwart.* HUGO SPITZER. Leipzig and Wien. 1897.

I. With Groos' book, 'Die Spiele der Thiere,' Lange conceives a new epoch of æsthetic study begun, because finally it is to be based securely upon the evolutionary hypothesis. The play instinct has been shown to have an important place in Natural Selection, and by that means the æsthetic consciousness has been brought directly into relation with the development of the species.

But, that the important relation of the play instinct to the æsthetic consciousness be properly understood, it is necessary that the distinction of play phenomena from other activities be carefully drawn; and, secondly, that the psychological nature of the play consciousness be clearly defined. Otherwise the theory of the importance of play in development may rest upon a too inclusive concept of play and our connection of the play consciousness with æsthetic upon insufficient psychological data.

As to the first, Lange criticizes the tendency of Groos to include too much under the concept of play. For instance, in the consideration of the plays of illusion, which is the kernel of the problem, the line between 'play' and 'earnest' is not clearly enough drawn,[1] although the importance of its distinction is recognized. Accordingly the plays (so called by Groos) which are a direct and purposeful bringing into activity of the parental and love instinct and all that leads to the gratification of the same, are, according to Lange, really not plays. Neither the acts of winning the other sex, nor the building acts that prepare for the reception of offspring can be classed as play. Nor can we call the torture of one animal by another, nor the fight for life

[1] Cf. Prof. Baldwin's criticism of Groos in *Science*, Feb. 26, 1897.

and death, illustrated in one case, by the play of a cat with the mouse, or, in the other, by the fight of two steers for a mate, play phenomena, as does Groos. All these phenomena can only be classed as play so long as they are of the nature of an imitation of the real, when the sense of simulation and illusion is present. As soon as illusion vanishes, earnest reality takes its place.

The temptation to confuse play and earnest has arisen out of the desire of bringing the concept of play under the rubric of the 'useful,' thus making it amenable to the operation of Natural Selection, entirely unnecessarily, as Lange thinks, if the real notion of illusion be kept in mind. For it is just in the fact that through conscious illusion instincts *are played with*, without leading to the practical results of the exercise of the instincts themselves, that the great value of play in the individual and the species may be seen. By means of play the instincts are brought into activity without going to the lengths of reality. Such instincts as are important for the promotion of the species are thus kept constantly in exercise, while, if used in reality, the exercise would be infrequent, owing to the weakness of the young and the dangers and natural opposition of the environment. "Die Thiere spielen nicht, weil sie jung sind, aber sie haben eine Jugend, weil sie spielen müssen"—in the words of Groos.

In this way Lange sees the doctrine of *bewusste Selbsttäuschung* brought under the concept of Natural Selection, and finds in it (agreeing with Gross?) ground for a modification of our idea of the struggle for existence. Through this illusion, play becomes the means of raising the simple reflex instinct to the first steps of intelligence, in that play is the first sense of new modifications of the instincts.

And now the problem of æsthetics. Can the same argument that gives to play such weight in the development of the animal, be equally cogent in giving the æsthetic consciousness a ruling place in the development of the human species. On the basis of the similarity of the play instincts with the æsthetic consciousness, which must be here assumed, though fully treated elsewhere by both authors, the conclusion of Lange, that æsthetic feelings play a large rôle in the development of the race, seems justified. As there are certain instincts whose exercise is necessary to the maintenance of an animal species, and for which the proper exercise can be obtained only through simulation in play, so there are social instincts, courage, patriotism, ambition, etc., necessary for the maintenance of society, whose strength and exercise is constantly kept up by outside simulations and representations of the same. Man needs art just as animals and children

need play, to preserve alive the feelings which tend to the maintenance of the social organism.

II. Spitzer's collection of short critical essays upon books that have recently appeared in the æsthetic literature of Germany serves equally well the purposes of introduction to, or criticism upon the authors with whom they deal. The first, '*Vom characteristisch Schönen*' and the fourth '*Die Entwickelung von Schiller's Æsthetik*,' are of interest for the historical problems of æsthetics. The second essay deals with a recent book of A. Biese, '*Die Philosophie des Metaphorischen*,' written in the spirit of Feuerbach's '*Theogonie*,' and attempting to give an account of the metaphor in art, religion and philosophy. The criticism points out a failure common to most psychological attempts of this nature, and one which Feuerbach himself did not escape, a failure of definition, in which the boundaries of the metaphorical are drawn too widely and include much which, through the abstraction and criticism of centuries, has taken on an intellectual and technical cast. The criticism, in the third essay, of the æsthetical portion of Dessoir's '*Geschichte der neueren deutschen Psychologie*,' deals with that radicalism of historical interpretation which many of its readers feel to be characteristic of the entire work. The last essay has for its subject a work of immediate interest in K. Lange's '*Die bewusste Selbsttaüschung als Kern des aesthetischen Genusses.*' Spitzer considers the theory in no wise equal to the Hutcheson-Zimmermann doctrine of the characteristic as an explanatory æsthetical principle, and finds it subject to numerous psychological doubts.

The test case of the illusion theory is, however, its application to the non-imitative arts, architecture, lyric and music. Here, of course, there can be no illusion in the sense of comparison of image with the real object, and to fill up the break the idea of '*Schein-Gefühle*' is introduced. Lange points out, especially for architecture, that there are certain static and dynamic feelings of pressure, strain, etc., which we put into the art object, but which in reality are not there. This idea of an illusion existing between the feelings and the object, Spitzer, as well as others, finds untenable. All feelings, then, are elements of reality, and the expression, '*Schein-Gefühle*,' is a *contradictio in adjecto*. The objection is justified to this extent: that it is a fault of expression to say that feelings are an appearance, and therefore, can be the source of illusion. All feeling is immediately given as real. The illusion arises when from these feelings there develop *vague ideas* of forces which we read into the art object, but which in reality are not there. The apparent difficulty is solved when

we consider that the feelings are part of the whole reality of the art object, the illusion is between this reality and the vague ideas we apply to it, between the ideas of external forces and our feelings.

The second difficulty which Spitzer finds in the idea of oscillation between semblance and reality, is the same which presents itself to Groos in 'Die Spiele der Thiere,' *i. e.*, that in many artistic intuitions the subject is entirely sunk in contemplation and no question of appearance and reality arises. From this fact Spitzer draws the conclusion that the illusion theory is itself faulty, while Groos simply directs the attention from the oscillating nature of the process. The truth seems to be that the degree of illusion, as well as the question whether it is continuous or an oscillation between appearance and reality, depends very much upon the nature of the arts under consideration—that is upon the relative freedom of the imagination and upon the number and character of the moments which tend to disturb the illusion. Music, with very few disturbing moments and a maximum of freedom, allows of long sinking of oneself in the illusion. On the other hand Painting, in which the critical faculty finds many moments to disturb, shows more of the process of passing to and fro between reality and illusion.

In conclusion it should be said that Spitzer finds in the illusion theory, as Lange also suggests, only one of several principles of explanation of æsthetic phenomena, and calls attention to the necessity of more accurate psychological research—especially in the spheres of natural beauty and the minor arts, to which the champions of the theory have not found it necessary to turn their attention.

WILBUR M. URBAN.

PRINCETON.

LOGICAL.

Über die Scheidung von grammatischem, logischem und psychologischem Subject resp. Prädicat. A. MARTY. Archiv für systematische Philosophe, 1897. 174–190 and 294–333.

The writer's purpose is to refute the generally received doctrine that there may be a fundamental discrepancy between the logical (or psychological) and grammatical elements of a judgment. By logical subject or predicate he means the elements of the thought itself, and by grammatical he means the expression of these in words. Two kinds of discrepancy, indeed, he admits at once: first, where the thought is not completely expressed, but where an element is indi-

cated by a gesture or left to be understood by the hearer; and, second, where there is in the proposition a *seeming* (scheinbares) subject or predicate, without any element in the thought corresponding to it. Under the latter class he includes existentials (put in the form but not having the value of categoricals), and categoroids (negatives like 'green is not red' which have the value of negative existentials, there is no red that is green).

The writer denies, however, that there are cases where the logical and grammatical elements are all present, but do not correspond to each other, the logical subject (or predicate) being expressed by a word which fulfills in the sentence quite a different function. In defense of this position he introduces a searching criticism of several writers, chief of whom is Benno Erdmann. Erdmann holds that the logical subject and predicate have no reference to their syntactical expression, but are determined solely by the objective relation of the ideas in the judgment, a relation he calls one of logical immanence. This relation is that of substance and accident, or an extension of it by analogy; it provides a rule for the distinction of logical subject and predicate in the content of the judgment. ('To the brave belongs the world;' logical subject, 'the brave.') If this is true, of course any judgment may have many forms of expression.

The writer, however, denies that Erdmann's rule is fundamental. As the most fundamental distinction he makes the subject the better known, the predicate, what is new. But other distinctions, originally or usually coinciding with this one, become established through custom, and often come finally to conflict with it. Among such distinctions are those of whole and part, substance and accident, first and last, and many more; and which one shall in any case prevail depends on custom and circumstances. The writer's point is that it is just these varying conditions which are brought out in the grammatical structure of the sentence, and that the syntactical functions of this structure do correspond to the logical functions of the thought.

To the objection that the sentence actually chosen often fails to express the exact judgment of the speaker, or even awakens a judgment different from itself in the mind of the hearer, the writer replies that the judgment expressed is responsible neither for other judgments in the mind of the speaker which he should but does not express, nor for judgments which the hearer afterwards reaches by association or inference. The sentence on the whole is an adequate expression of the thought below it, and grammatical relations stand for logical ones.

While this conclusion is well made out, the writer introduces

Brentano's distinctive view of judgment without any bearing on his main theme, and it would seem without adequate support. He has an interesting section on the origin of the grammatical consciousness, a subject which has been passed over too lightly even by the newer logicians. The paper, on the whole, marks a forward step.

J. FORSYTH CRAWFORD.

CHICAGO.

BIOLOGICAL.

La Structure du Protoplasma et les Théories sur L'hérédité et les Grands Problèmes de la Biologie Générale. Y. DELAGE. Paris: Reinwald, 1895. Pp. xiv +878.

Thirty years ago this title would have suggested that the book was a hopeless hodge-podge. Now that we see more deeply into the relation of things we recognize that the author could hardly have dealt with less and treated it completely. For, on the one hand, it is clear that heredity depends upon the specific constitution of the protoplasm, has to do with the causes of ontogenesis, and, combined with variation, makes possible evolution. On the other hand, the structure of protoplasm has no meaning apart from heredity, individual development, and phylogeny, so that the modern text-book on the cell must consider its significance in development and inheritance. The cell, the individual, and the race are merely units of different order in the world of living substance.

In the book before us Professor Delage has preserved a very satisfactory balance between the facts concerning the cell, the individual, and the race (300 pages) and the theories which have been offered to group and explain them (500 pages). He has put himself into every page, so that the book is nowhere a mere compilation; but, more than that, his extensive review has enabled him to render valuable judgment upon theories and to offer a highly satisfactory explanation of the cause of phylogenetic differentiation.

A glance at the table of contents will best reveal the broad scope of the book. The cell: Its constitution; its physiology; its reproduction, including the relation of nucleus to cytoplasm. The individual: Regeneration; grafts; generation by fission and budding, sexual and asexual reproduction; ontogenesis; metamorphosis and the alternation of generations; sex and the secondary sexual characters; latent characters; teratogenesis; correlation; death, immortality and the germplasma. The race: Transmissibility of characters innate and acquired

- **I. ANIMISTS**
  1. *Soul.* — *Plato*, Saint Augustin, Van Helmont.
  2. *Nisus formativus.* — *Blumenbach*, Needham.
  3. *Vital force.* — Barthez, Bordeu, Lordat, old school of Montpelier.

- **II. EVOLUTIONISTS**
  1. *Spermatists.* — Erasistratus, Diogenes of Laërte.
  2. *Ovists.* — Galen, Leuwenhoek, Andry, Dalempatius, Harvey, Graaf, Swammerdam, Malpighi, Haller, C. Bonnet, Spallanzani, De Blainville

- **III. MICROMERISTS.**
  - 1. Universal, immortal particles.
    - *Organic molecules.* — *Buffon.*
    - *Microzymas.* — *Béchamp.*
  - 2. Particles which are destroyed after death.
    - **A. All of the same nature; all exercise, with equal share, their influence in the deter. of all the parts. Active:**
      1. By their polarity.—*Physiological units: Polarigenesis.* — *Spencer.*
      2. By their form and their molecular forces.—Gemmæ and Gemmaria. — *Haacke.*
      3. By their vibratory movements.
         - *Annular atoms* — *Dolbear.*
         - *Conservation of the Plastidules* — *Erlsberg*
         - *Perigenesis of the Plastidules* [*of the germ.*] — *Hæckel* } PERIGENISTS
         - *Undulatory propagation of the organogenic excitation to the limits* — *His.*
         - *Kinetogenesis and Catagenesis.—Bathmism and Catagenesis* — *Cope.* } PERIGENESIS AND ITS VARIANTS.
         - *Morphogenic action of habitual functioning* — *Orr.—Mantia.*
    - **B. of different species and charged with diverse functions.**
      - **Non Representative:**
        - a. simple chemical molecules. Active:
          - a. By their physico-chemical properties. — *Hanstein.* *Berthold.*
          - b. By their pure chemical properties. — *Chevreul, Geddes, Thompson, Gautier, Danilevsky.*
        - b. Aggregates of a higher order.
          - a. Electric apparatus. — *Fol.*
          - b. Chemical apparatus. — *Maggi.* *Altmann.*
          - c. Initial particles endowed with vital properties. — *Wiesner.*
      - **Representative:**
        - a. Of *Ancestral plasms.* — *Weismann* (in theory).
        - b. Of cells of the body.
          - *Gemmules* — *Ch. Darwin*, PANGENESIS, S. S.
          - *Germs, representative of organs* — *Maupertuis* } PRECURSORS.
          - *Filaments with appetence and molecules with propensities* — *Erasmus Darwin.* } PRECURSORS.
          - *Stirpes* — *Galton,*
          - *Odoriferous gemmules* — *Jaeger,*
          - *Female germs and male germs* — *Brooks,*
          - *Cytozoaires* — *Gaule, Platt-Ball* } VARIANTS.
          - *Stereometric structure of protoplasm* — *Hallez,*
          - } PANGENESIS.
        - c. Of elementary characters and properties of the organism.
          - *Micelles, Idioplasma* — *Naegeli.*
          - *Nuclear idioplasm* — *Kölliker.*
          - *Intracellular pangenesis* — *De Vries.*
          - *Idioblasts* — *O. Hertwig.*
        - d. At the same time of the parts of the body and the elementary characters. — *Weismann* (2d theory).

- **IV. ORGANICISTS.** — *Descartes*, von Baer, His, Bichat, Cl. Bernard, *Roux* (Driesch, O. Hertwig.)

and of transitory states in the parent; concerning telegony and xeny; heredity in asexual and sexual generation, in close unions, in hybridization, and in grafting; variation, its sorts and its causes; the facts concerning the origin of species. In the second part, a hundred pages are devoted to theories relating to the foregoing phenomena. In the third part, the general biological theories are explained and criticized. These have been classified by Delage as in the accompanying table.

The author's own 'theory of actual causes' is now brought forward. The course of ontogenesis depends not only upon the constitution of the germinative plasma, but also upon tropisms and tactisms, functional excitation and the various external conditions of development. The origin of adaptation in species is not due to the summation by selection of favorable individual variations for, as G. Pfeffer has pointed out, the killing off in selection generally occurs during immaturity so it can hardly determine the adaptation of the adult. Delage believes that sports only exceptionally form species, at least they cannot account for the adaptation so characteristic of species. The adaptive specific qualities which any theory of the origin of species must recognize and explain are produced as follows: Species are variations become fixed. Adaptive variations are brought about by self adaptation (or accommodation) of the individual under the influence of functional excitation. When the conditions under which development occurs change, the individual adapts itself to the new excitations it encounters. But how does this adaptation in the individual bring about an adaptation in the species? There is, strictly, no species adaptation but only individual adaptation. Let us assume a change from any cause in the germ plasm. This change is adaptive or non-adaptive. If adaptive so much the better for the individuals; if unadaptive the individuals will not all die off, but "the individual efforts will be more energetic and more sustained, the somatic adaptation will be perfected by a more energetic functional excitation, a certain number of individuals will, without doubt, succumb among the less plastic or the more delicate, and thus the auto-regulation of the mean number of individuals of the species will be effected, but the species will continue none the less to live. It is only when the variation is radically pernicious that it will succumb. Usually the variation which (through correlation) affects necessarily, although to very diverse degrees, all parts and all functions, will be injurious for some, advantageous for others, indifferent for most, and an (individual) compensation will be established which will make the injurious pass under the protection of the advantageous."

A word concerning the place of Delage's theory. It is confessedly a descendant of Roux's. It is also closely related to George Pfeffer's theory,[1] differing chiefly in that it lays less stress upon the selection of plasticity in the organism.

Enough has been said to indicate that Delage's book is valuable, not merely as an indispensible encyclopœdia of facts and bibliography, but as a substantial contribution to theoretical biology.

C. B. DAVENPORT.

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The Average Contribution of Each Several Ancestor to the Total Heritage of the Offspring. FRANCIS GALTON. Proceedings of the Royal Society. Vol. 61, 401-413. (Read June 3, 1897.)

In his work on 'Natural Inheritance' (1889) Dr. Galton stated tentatively, that the influence of each parent on its offspring would be one-fourth, of each grandparent one-sixteenth, etc. This result was deduced from a discussion of his data on human stature, and he announced at the time that for the purpose of testing it he had in progress experiments on moths. In the paper before us Dr. Galton states that the experiments on moths failed, but that he has found excellent

[1] Pfeffer's theory has such points of similarity to that recently proposed by Baldwin, that it may be worth while to translate here Pfeffer's own summary of his theory, which is to be found in the 'Verhandlungen des Naturwissenschaftlichen Vereins in Hamburg,' 1893, pages 44 to 87.

"The struggle for existence rejects all pernicious individuals and lets survive some individuals belonging to the average of their race; changes in the external conditions of life change the species, since they change the average of the surviving individuals, impress thus upon the mass of the species a different facies, and permit them to appear, alongside of their relatives, as a different race, variety or species. The remaining part of the Darwinian theory, namely, the gradual production of new races and species, seems, consequently, unnecessary; the pristine characteristic Darwinian principle of the survival of fitness suffices for the comprehension of the form-changes with which we are concerned."

I have translated by 'survival of fitness,' the phrase, 'Uberleben des Passenden.' It is possible that 'fittingness' or capacity for accommodation would have given the idea better. This conclusion would seem to be justified by the following words of Pfeffer on page 71: "Von allen jungen Tieren gehen unbedingt diejenigen zu Grunde, deren körperliche Verhältnisse *nicht* zu einer praktischen Handhabung der erforderlichen Eigenschaften für das jeweilige Leben führen; der Kampf ums Dasein merzt die mangelnde Geschicklichkeit und damit die mangelhafte körperliche Grundlage und deren Besitzer aus. Also führt die Veränderung der äusseren Lebensbedingungen zu einer immer weiter fortschreitenden Ausbildung körperlicher Verhältnisse, welche eine geschicktere Bewegung des betreffenden Tieres unter den neuen Bedingungen ermöglichen."

material in the records of a pedigree stock of Basset hounds. These hounds have two, and only two, recognized varieties of color—transitional cases being very rare—which may be called T (tricolor) and N (non-tricolor). With 817 cases at his disposal, Dr. Galton could trace the ancestry of the hounds and determine the influence of the parents, of the grandparents, and (in 187 cases) of the great-grandparents, on the offspring. The results confirm his principle with surprising accuracy. Thus, for example, in the simplest case, where one parent and two grandparents were T, one-half the offspring (subject to a slight correction for an excess of T in the great-grandparents) should be T, and of 60 cases 36 were T. When all the parents and grandparents were T, of the 119 cases 108 should be T, and 106 were in fact T. The grand totals give in the cases where the grandparents were known 387 T, as compared with a theoretical 391, and in those cases where the great-grandparents were known, 181 T, as compared with a theoretical 180. In this special case the hypothesis is fully confirmed, and it may fairly be called a law of heredity. The law is stated: "The two parents contribute between them, on the average, one-half or (0.5) of the total heritage of the offspring; the four grandparents, one-quarter, or $(0.5)^2$; the eight great-grandparents one-eight, or $(0.5)^3$, and so on."

It should, however, be noted that in Dr. Galton's material we have a trait that must be present or absent, and is normally present in about one-half of all the cases. I am not sure that he is justified in extending the law generally to human, animal and plant heredity. If the Basset hounds were crossed with mongrels one-half of the offspring would not be T or N. Dr. Galton has himself argued that variations obtained by artificial selection tend to revert to the racial mean, even when maintained for a long series of generations, and individuals having such variations cannot influence the offspring as much as is required by this law. I should suppose that the greater the departure of the parent from the mean of the race, or the more rare the variation, the less, as a rule, would be its potency in heredity. It seems to me that the stability of variation must, in each case, be determined by observation or experiment, Dr. Galton's law being too simple to fit the complexity of nature.

J. McKeen Cattell.

Columbia University.

VOLITION AND GENERAL.

Voluntary Action. G. F. STOUT. Mind, July, 1896. Pp. 354–366.

Types of Will. ALEXANDER F. SHAND. Mind, July, 1897. Pp. 289–325.

In an article in *Mind*, October, 1895, Mr. Shand maintained that will, though analyzable up to a certain point, had, in the last resort, a distinctive quality, incapable of further analysis or description. Mr. Stout here offers as an alternative the theory that will is desire qualified and defined by a certain sort of judgment, the judgment, namely, "that, so far as in us lies, we shall bring about the attainment of the desired end." The characteristic difference between indecision and decision is that in the former we do not yet know what we are going to do, while in the latter we do. Mr. Stout explains by means of this conception the distinction between voluntary and involuntary action which Mr. Shand made so much of. Thus, *e. g.*, the sneeze of a soldier marching to surprise a fortress is involuntary, because, although foreseen, it is not foreseen as something he desires to bring about; the indulgence of a morbid appetite may express the volition of the moment, but may be regarded as involuntary with reference to the man's general volition, etc.

In what is virtually a reply, but has the form of an independent essay, Mr. Shand endeavors to show that Stout's theory like all other general theories of the will, fails to take proper account of different types of volition. Urging the necessity of studying these before resting in any one general formula, he distinguishes and analyzes a number of volitional types and arrives, substantially, at the following conclusions: (I) *Simple volition.* We first seem to have will, as distinguished from inferior conations, when, along with the idea of a desired action, we have the judgment that, as far as in us lies, we are going to realize it. This agrees with Mr. Stout's formula. Simple volition is the state described without doubt or conflict of motives; complex volition is the state described preceded by such doubt or conflict. (II) *Will as negation.* The above definition includes only positive judgments. But there are volitions with the negative judgment, I am not going to do this. And this is a distinct type; for though logically the negative judgment implies the positive, this is not true psychologically. The psychological accompaniment of negative volition is not necessarily a contrary positive judgment, but a contrary positive conation. We must accordingly modify our definition and

say that the distinguishing character of will is either a judgment that we are, or a judgment that we are not, going to do something, or it is a mixture of both judgments. (III) *Hypothetical and Disjunctive Will.* But beside volitions of the categorical type, there are hypothetical and disjunctive volitions; and the peculiarity of the former is that they do not affirm that we are going to do anything, while the latter affirm that we are going to do one thing or another. Thus volitional judgment may assume a variety of forms, categorical, hypothetical, disjunctive, positive and negative. But the characteristic of will is not in the form of the judgment, but in the content, namely, in the emphasis on the agency of the self (which is, however, not peculiar to will), and in the belief that, conditionally, or unconditionally, we shall try to do (or not do) something. (IV) *Fictitious Choice.* Complex volition, or choice, is so defined that the following types must be taken to represent not real, but fictitious choice. (1) A traveler already decided to take the shortest road, on learning that this is the shortest, decides to take this. Here the antecedent conflict is purely intellectual. (2) He had not previously decided on the shortest, but on learning which is the shortest, at once decides to take that. Here blind conation develops into will without any conflict of desire. (3) A youth in easy circumstances determines to choose the profession for which his inclination is strongest, but is in doubt as to which is his strongest inclination. Until the final decision, there is conflict of desires, but no conflict of motives; as in (1) the volition becomes definite, but is essentially unchanged. (4) A child is set to choose between two playthings. If we assume a blind conation, at the start, to choose 'the nicer,' then, here too, in spite of the conflict of desires and apparent motives, the final volition is only the development of the original conation. In all four types, there is no real conflict of motives, consequently no real choice. The judgment is made up and defines the conation; but volition may run counter to the judgment, choose the worse, etc. (V) *Involuntary Action.* The most interesting cases are ideo-motor actions. (1) Actions produced suddenly through fear. If we maintain Mr. Stout's definition of an involuntary action and require the simultaneous existence of a voluntary resolution, such actions will have to be regarded, not as involuntary, but only as non-voluntary. (2) Actions produced by fear, but preceded by a determination not to do them. (3) Types in which conflict of desire is present, *e. g.*, the soldier endangering his own life and the army's by coughing. This case shows plainly that the distinctive constituent of will cannot be the judgment that we are going to do something. (4) Possibly types

involving choice; but an unambiguous case is hard to find. (VI) *Will as Imperative.* So far from it being true that volition is essentially determined by the judgment, I am going to do something, some volitions are expressed, not by a judgment at all, but by an imperative. Such imperative volitions always have for their object the control of another's conduct, and though usually simple, they may, by sympathy, be complex and involve choice. This type cannot be eliminated by assuming that imperatives are merely means for getting preformed volitions accomplished, for in some cases the conation issues in the imperative too suddenly. Nor can it be resolved into any of the judgment types; it is a unique differentiation. The judgment is either true or false, the imperative is neither true nor false. Finally (VII) *Desire and Will* do not always have the relation ascribed to them by Mr. Stout. Will is sometimes determined, not by desire, but by the less strong aversion. Thus the condemned man allowed to choose the form of his execution, actually wills to do what he desires not to do, since he desires to escape death in every form. There are in fact three types: (1) desire is the motive; (2) desire is effaced from the motive (*e. g.*, 'duty for duty's sake'); (3) desire is replaced by aversion.

Issue may fairly be taken with some of these contentions. If, *e. g.*, the condemned man choose to be shot rather than hung, it seems incorrect to say that his will is contrary to his desire because he does not desire to be shot. He does not will to be shot simply, but to be shot rather than hung. He is averse to being hung, he is also averse to being shot; but he has, among other stronger desires, this desire also, to get through with the disagreeable necessity in the least obnoxious way possible under the circumstances, and he resolves accordingly. In regard to 'imperative volition,' it may be doubted whether, *e. g.*, the command 'Do this' expresses more than a mere wish or desire apart from the implied consciousness, "I am determined that, as far as I can control your conduct, you shall." Finally, as regards the interpretation of the soldier's involuntary cough or sneeze, it seems beside the mark to say that it proves that the fore-knowledge, I am about to do this, is not the essential character in will; for nobody, certainly not Mr. Stout, said that it was.

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Genesis of Number Forms. D. E. PHILLIPS. Amer. Jour. of Psychol. VIII., 4, p. 506. July, 1897.

This study is noticeable for the fulness of its material, comprising returns from about two thousand persons. Half of these (974) were

from children of ten to sixteen years in the Worcester grammar schools, and one-sixth (332) were from students in a normal school. Most of the children were privately questioned and precautions were taken against their 'imagining forms for the occasion.' The writer of this notice, from her own experience, cordially endorses the conclusions of Mr. Phillips, from this verification, agreeing with him that "after giving the slightest explanation, a close observer will hardly fail to distinguish every one having distinct number forms. Those who have no form," the author adds, "have no idea of what you are speaking. * * * Those having a form show an entirely different attitude."

The most significant result of the paper is the conclusion of Mr. Phillips that the possession of mental forms is no sporadic aberration of a few individuals, but merely the pronounced manifestation of a very general characteristic. "There is no more reason," he says, "for isolating these mental activities from a much larger field, than there is for isolating exceptional cases of memory or imagination from these general powers of the mind." The statistics of the study do not at first sight lead to this result, for only sixteen per cent. of the subjects claimed a number form, when originally questioned. But the attention of Mr. Phillips was attracted by the experience of Dr. Story who "denied that he had a number form, but remarked that large numbers appeared far off." This led to a re-examination of 250 of the adults of the former investigation who had denied having a form, and to the discovery that 210 of these "have a feeling that numbers in some way recede from them."

This result, as Mr. Phillips suggests, not only shows that "nearly all persons possess some idea of extension of numbers, more or less indefinite," but it throws some light on the baffling subject of the psychology of numbers. The fact that the most primitive number-form seems to be a 'sensation of following in some particular direction' allies the numerical series with the tendency of motion. The number-form is thus an indication of the close connection between the motor and the spatial image, and between the arithmetical and the geometrical unit.

The universality and the thoroughly 'normal' nature of the number-form is indirectly suggested by other results of the study with which, in general, the statistics of similar investigations by the writer of this notice very definitely agree. In the first place, all those who remember the origin of these forms refer them to ordinary experiences in learning to count and to read (p. 514). Furthermore, inquiry

fails to reveal a greater proportion of forms among the 'intellectually active,' or the 'imaginative,' which suggests that the form is not the adjunct of the riotous fancy merely. The permanence of forms is shown by the discovery of 14 per cent. among adults, as over against only 18 per cent. among children. Finally, the utility of forms points to their general occurrence, and 97 of the 212 who answered the questions of Mr. Phillips are sure that forms are helpful in the mental life, while only one counts them 'troublesome.'

The study of Mr. Phillips is valuable, therefore, because it tends to lure the number-form from the *terra* more or less *incognita* of the abnormal, into the familiar domain of the normal psychic life.

MARY WHITON CALKINS.

WELLESLEY COLLEGE.

Sull'Importanza delle Ricerche Relative alla Storia delle Scienze. GIOVANNI VAILATI. Torino, Roux Frassati e Co. 1897. Pp. 22.

This is a lecture introductory to a special course upon the history of mechanics. The author insists that an intimate knowledge of the historical development of a science is absolutely necessary to a thorough understanding and right appreciation of its present day methods and results. By many historical instances he shows how the men of one generation have been indebted to the labors of those of preceding generations, for methods of observation and experiment, for proved and established principles and laws, for working formulæ, and for a vast and ever increasing accumulation of classified facts, and arranged material. He illustrates this dependence upon the past by references especially to the history of mathematics naturally leading to a special disquisition upon the development of the science of mechanics, the latter being the author's objective end in view throughout this introductory lecture. He draws attention to the fact that in the European universities there is an increased number of courses offered this year in the history of the various sciences. This signifies the importance which is now attached to historical research as an aid in the present development of science.

JOHN GRIER HIBBEN.

PRINCETON UNIVERSITY.

Die Assoziationsfestigkeit in ihrer Abhängigkeit von der Verteilung der Wiederholungen. ADOLF JOST. Zeitschrift für Psychologie u. Physiologie der Sinnesorgane, XIV., 6. pp. 436–472.

This paper gives an account of experiments carried on in the Göt-

tingen Psychological Institute with the view of investigating the relative value of distribution and summation of repetitions in the process of memorizing series of nonsense syllables. The experiments were made with series of 12 syllables constructed according to the method employed by Müller and Schumann in their research on memory.

The first experiments were directed to the more exact investigation of a point on which some experiments of Ebbinghaus had already thrown light, viz., the advantage of distributing over several days the total number of repetitions employed in learning. In one group of experiments the series (*Cumulationsreihen*) were repeated 30 times on one day, and learned by heart next day; in the other group the series (*Vertheilungsreihen*) were repeated 10 times on each of 3 successive days and then learned by heart on the fourth. The result was that with both the subjects on whom the experiments were made the number of repetitions required for learning the series by heart was about 15% less in the second group than in the first. The next set of experiments was designed to show whether the lesser effectiveness of the accumulated repetitions was due to fatigue or to some specific new factor. The repetitions in this group (24 in number) were so arranged that the fatigue incident to persistent repetition was distributed equally over the *Cumulations* and *Vertheilungsreihen* which were read in the same hour. Here the advantage on the side of distribution was not so great, but was still quite marked.

The next step consisted in testing the value of various forms of distribution, three different forms being tried, viz., 2 repetitions on 12 days, 4 on 6 days and 8 on 3 days. Here the strength of association was tested by a new method (*Treffermethode*). Six syllables from each of the series which had been already memorized were presented in succession to the subject, who was required to name the syllables which had followed these in the original series; the reaction time for each reproduction was, at the same time, determined by means of Hipp chronoscope and lip-key. It was found that the number of syllables rightly named increased progressively with the extent of the distribution, being greatest where the 24 repetitions were spread over 12 days. The reaction time was longest for the series spread over 3 days; the effect of distribution, however, seemed to be partly obscured by the fact, which has been established in the Göttingen laboratory, that older associations, though more correctly reproduced, have longer reaction times than those more recently formed. The attempt to measure the value of varying extent of repetitions by any known method is declared by Jost to be impossible. In regard to the method em-

ployed by me in the research on 'The Place of Repetition in Memory,'[1] he remarks that any given number of repetitions has its effect not only in the reproduction of certain syllables, but in a certain '*Hebung der Bereitschaft*,' of others which are not reproduced; this latter factor, however, we can '*im einzelnen Falle garnicht beurtheilen*.' It is true that we cannot in any particular case measure this supposed *Hebung;* we cannot well do it in general, for it is something which remains below the threshold of consciousness. But this does not destroy the importance of the fact which the Münsterberg method has established, viz., that the number of syllables which are actually reproduced bears a definite relation to the number of prior repetitions.

The next experiments were devoted to proof of the proposition that if two associations are of equal strength, but of different ages, a new repetition has a greater effect on the older of the two associations. A number of series *(alte Reihen)* repeated 30 times on one day were compared next day by the *Treffermethode* with other series *(junge Reihen)* repeated four times during the hour in which the first series were being tested; the first series, it was found, gave fewer right syllables and took longer reaction times than the latter series. But it was found at the same time that with other series arranged in the same way, but tested by the number of repetitions required for learning by heart, the 'old' series required nearly 40% fewer repetitions than the 'young' series. The last question which was experimentally investigated was more practical: what is the most economical way of learning a series? In one set of experiments the series were repeated 4 times each day, while in another set they were repeated twice, and this was continued with the series until each was learned. The result was not very decided, but, so far as it went, tended to show that the advantage lay with the wider distribution.

As may be seen from the foregoing report, the experiments furnish a valuable addition to the scientific knowledge of the laws of memory. Their value for educational science not merely in direct result, but in the promise of further development, is no less evident.

SMITH COLLEGE. W. G. SMITH.

Beiträge zur speciellen Dispositionstheorie. STEPHAN WITASEK. Archiv für Systematische Philosophie, III., pp. 273–293.

Beiträge zur Psychologie der Komplexionen. STEPHAN WITASEK. Zeitschrift für Psychologie u. Physiologie der Sinnesorgane, XIV., 6, pp. 401–435.

[1] This REVIEW, III., p. 21.

In these two articles we have contributions to the development of various elements in the psychological theory of the school which has Meinong for its leading representative. In the first article Witasek discusses the classification of dispositions; in the second the chief subject is the formation of *Komplexionen höherer Ordnung*, of more complex synthetic ideas.

"The modern concept of disposition," says Witasek, "has for its content * * * the causal relation between a relatively persistent property of the active subject *(Dispositionsgrundlage)* as cause and its product (*Dispositionscorrelat*) as effect." It is not the disposition, but its basis, the property of the subject, which has reality. Modern physiology and psychology are recognizing the existence of dispositions; it is of great importance to attain to exact notions regarding the dispositions whose interplay is evident in our mental life. Psychology has long recognized that perceptions are the correlate of an *Empfindungsdisposition*, and that ideas or memory images are the correlate of a *Reproductionsdisposition*. There is also to be assumed a disposition corresponding to the new form of psychical content present in the *Komplexionen*, or complex ideas. But these complex ideas may arise in the mind either by way of direct construction—first, the constituent elements, then the new connecting element—or indirectly, in which case the relation is the primary object, the content which is related arising at the same time. Common examples of the latter fact are found in our recollection of complex related content. It can be seen in this case that, when once the relating synthetic activity has been operating, a disposition to the renewal of that activity will be formed. But analysis becomes more difficult when we consider the complex forms of combination involved in imaginative construction. It is not sufficient to adduce here as explanatory factors reproduction of unrelated content and subsequent relating activity; for, in that case, the characteristic feature would be neglected—the presence as a primary object of the form or ideal outline which becomes filled up by further activity. We must assume that in imagination or phantasy we find the operation of a new specific disposition.

Having thus given his grounds for assuming three dispositions in the domain of *Komplexionen*, viz., the dispositions implied in the relating activity itself, in the reproduction of complex ideas and in imaginative construction, Witasek proceeds to the further questions, whether the relating activity can be intensified and whether it can lose its effectiveness. The proof for an affirmative answer to the first question is taken chiefly from the sphere of music; the beginner may easily

fail to understand a musical work in its connection and complete form, but with practice his power of understanding and appreciating may be greatly increased. In many lines of mental activity our perception of relations is already so thoroughly trained, when we begin to attend to it, that an improvement can with difficulty be traced. When a previously attained facility is lost we have to do in reality not with a loss of faculty but a loss of practice. The operation of such factors as exhaustion and recovery cannot be easily demonstrated; their presence is, however, highly probable.

In the introduction to the second paper Witasek takes up the question of perception of change in connection with the discussion on this subject at the Psychological Congress in Munich. In addition to indirect or ratiocinative perception of change Dr. Stern had seemed to distinguish two other forms, that in which all the factors of change are immediate contents of consciousness—direct or specific perception of change—and that which is completed in momentary perception. According to Witasek the last two forms are fundamentally similar, the essential fact in both being the presence, in addition to the changing sensational content and in inseparable union with it, of a new synthetic content which has no correlate in the series of physical stimuli, and which is in reality of the same character as the *fundierte Inhalte* for which Meinong and others contend.

The proper object of the paper is the investigation of two problems in regard to the *Komplexionen*. The first problem is this: how are we to explain the fact that out of the unconnected manifold of sensations which we receive in experience certain elements are singled out and combined with others in quite definite complex ideas? Take a complex musical work for example: the manifold of tones which is heard will be grouped and interpreted in various ways by different individuals. The perceived content does not give the complete reason for the different groupings. But if, on the other hand, we appeal to the activity of the subject for an explanation, we seem to be left in danger of subjective caprice. Often, indeed, the forms and connections of what is perceived seem fixed apart from choice; on the other hand, in such processes as comparing, relating and imaginative construction the subject is evidently an active participant. Where the subject apparently has no choice there are in the perceived content determining factors such as 'weight' of an idea and likeness among elements, which influence the attention and the relating activity. Where such moments in the complex of tones are weaker a certain effort is required to appreciate the music; here it is the function of a

subjective synthetic activity to bring to completion the connections which are obscurely indicated. It is in this activity that Witasek finds the true ground for the more complex ideal formations. Analysis plays its part in preparing the material for the relating activity. But where, as in music, we have to resist the wrong combinations which press themselves on our attention and expend effort in realizing the true interpretation, there it is evident that a specific synthetic activity of the subject is in operation.

The second discussion is devoted to a comparison of discrimination, as it affects on the one hand the constituent elements of a complex idea and on the other the form, shape or relation in the idea itself. From a consideration of further examples in the domain of music Witasek concludes that in the comparison of two complex objects the difference between the parts may be evident, and yet the difference between the forms and total relations of the two objects may be below the threshold. In some cases we seem to be able to manipulate and determine the complex forms more readily and securely than the parts, yet if we examine more closely we shall see that in no case do we really have perceptible difference of forms where the parts cannot be discriminated; what is lacking to the parts is only attentive analysis. In cases where, while noticing the difference of the parts, we wrongly assert an identity of the forms, there we are again misled by lack of analysis and by too great attention to common elements. Language often fails to give needed help to analysis.

This brief account of Witasek's argument will show in what direction the 'new way of ideas' is tending. The main characteristic seems to be a tendency to assume a new psychical content or activity where there appears a well marked feature or grouping of content which does not include in its immediate context all the conditions of its realization. But whatever their ultimate worth may be, such developments are valuable in calling attention to that neglected chapter, the psychological analysis of the more complex processes of thought and ideal activity.

W. G. SMITH.

SMITH COLLEGE.

NEW BOOKS.

Hypnotism and Its Application to Practical Medicine. O. G. WETTERSTRAND. Translated by H. G. Peterson. With letters on Hypno-Suggestion, by the translator. New York and London, Putnams. 1897. Pp. xvii+166. $2.

The Psychology of the Emotions. TH. RIBOT. English translation in Contemp. Science Series. London, W. Scott; New York, Scribners. 1897. Pp. xix+455. $1.25.

Hallucinations and Illusions. E. PARISH. London. English translation in Contemp. Science Series. London, W. Scott; New York, Scribners. 1897. Pp. xiv+390. $1.25.

Magic, Stage Illusions and Scientific Diversions, Including Trick Photography. Compiled by A. A. HOPKINS. With Introduction by H. R. Evans, and four hundred illustrations. New York, Munn & Co. 1897. Pp. xii+556. $2.50.

The New Psychology. E. W. SCRIPTURE. Contemp. Science Series. London, W. Scott; New York, Scribners. 1897. Pp. xxiv+500. $1.25.

Ethische Principienlehre. H. HÖFFDING. Bonn, Siebert. 1897. Pp. 64. M. .60.

L'Anneé Philosophique; Septième Anneé. 1896. F. PILLON. Paris, Alcan. 1897. Pp. 316. Fr. 5.

L'Évolution des Idées Générales. TH. RIBOT. Paris, Alcan. 1897. Pp. 260. Fr. 5.

I Riflesse vascolari nelle Membra e nel Cervello dell'uomo. M. L. PATRIZI. Reggio Emilia, Calderini. 1897. Rep. from Riv. Sper. di Freniat. Pp. 85.

Zur Psychologie der logischen Grundthatsachen. H. GOMPERZ. Leipzig, Deuticke. 1897. Pp. 103. M. 2.

Introduction to Philosophy. O. KÜLPE. Trans. by W. B. PILLSBURY and E. B. TITCHENER. London, Sonnenschein; New York, Macmillans. 1897. Pp. x+245. $1.60.

The Conception of God. J. ROYCE, J. LE CONTE, G. H. HOWISON, S. E. MEGES. Publications Philosoph. Union Univ. of California. Vol. I. New York and London, Macmillans. 1897. Pp. xxxviii+354. $1.75.

Darwin and after Darwin. III. Isolation and Physiological Selection. G. J. ROMANES. Chicago, Open Court Co. 1897. Pp. vi+181. $1.

Raumæsthetik und geometrisch-optische Täuschungen. TH. LIPPS. Schriften der gesell. f. psych. Forschung, heft 9–10 (II Sammlung). Leipzig, Barth. 1897. Pp. viii+424. M. 12.

The Subconscious Self and its Relation to Education and Health. L. WALDSTEIN. New York, Scribners. 1897. Pp. 171. $1.25.

Richerche psicofisiologiche sull' Attenzione. S. DE SANCTIS. Roma, Tip, Innocenzo artero. 1897. Pp. 48.

Il Tempo di Reagione Semplice. M. L. PATRIZI. Reggio-Emilia, Calderini. 1897. Pp. 15.

A Study of Puzzles. E. H. LINDLEY. Diss. for Doctorate, Clark Univ. (Rep. from Amer. Journ. of Psych.), VIII, 4. 1897.

The Impersonal Judgment. S. F. MACLENNAN. Diss. for Doctorate, Chicago Univ., Chicago Univ. Press. 1897. Pp. 49.

The Psychic Development of Young Animals. Cortical Cerebral Localization. The Functional Development of the Cerebral Cortex. WESLEY MILLS. Three papers reprinted from Trans. Roy. Soc., Canada. Vol. II., Sec. IV. 1896.

Sixteenth Annual Report of the Bureau of Ethnology, 1894–1895. J. N. POWELL Director, Washington, Gov. Print. Office. 1897. Pp. cxix+326.

Sleep: its Physiology, Pathology, Hygiene and Psychology. MARIE DE MADACÉINE. Contemp. Science Series. London, W. Scott; New York, Scribners. 1897. Pp. vii+341. $1.25.

Social and Ethical Interpretations in Mental Development. A Study in Social Psychology. J. MARK BALDWIN. New York and London, Macmillans. 1897. Pp. xiv+574. $2.60.

NOTES.

A LABORATORY for experimental psychology has been opened in the Illinois Eastern Hospital for the Insane, at Hospital, Ill., under the direction of Dr. W. O. Krohn, who has given up his position in the University of Illinois.

THE *Zeitschrift f. Psychologie u. Phys. der Sinnesorgane* is now published by Barth, Leipzig. The same firm will also publish Helmholtz' *Vorlesungen über theoretische Physik*, and the series of *Abhandlungen zur Physiologie der Gesichtsempfindungen*, edited by v. Kries, of which the first number has already appeared (M. 5).

FRANCIS KENNEDY, PH.D., Leipzig, has been appointed demonstrator in experimental psychology in Princeton University.

IN the current number (Bd. III., Heft. 4) of the *Arch. f. Syst. Philosophie*, there is issued a *Bibliographie der gesamten philosophischen Literatur* for the year 1896, comprising 1831 titles.

All communications for the editors of THE PSYCHOLOGICAL REVIEW, together with books, reprints, etc., intended for review, should be sent, during the year beginning November 1, 1897, to Professor J. McK. Cattell, Garrison-on-Hudson, New York.

INDEX OF NAMES.

The numbers are in *italics* for contributors; in heavy Roman type for the authors reviewed, and in thin Roman type for names mentioned in the notes.

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New and Forthcoming Books.

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From the *Philosophical Review*:—The gold medal of the Royal Academy of Denmark has been awarded in philosophy to Professor J. Mark Baldwin of Princeton. His memoir was in substance this work. . . . The following is an extract from Professor Höffding's Report to the Danish Academy.

"At the meeting of Feb. 8, 1895, the Royal Academy of Denmark put in competition the following question in philosophy (see the *Comptes Rendues de l'Académie*, 1895, p. 22): 'Is it possible to establish for the individual isolated in society a line of conduct drawn entirely from his personal nature; and if such rules are possible, what is their relation to the rules which would be reached from the consideration of society as a whole?' " . . . The memoirs seem to fall, according to their value, into three categories. . . . The third category contains a single memoir which is free from the defects which diminish the value of the others. By the method which it employs, it throws new light upon the solution of the question proposed and thereby advances it remarkably toward a solution . . . a memoir written in English, bearing the pseudonym of *Socius*. This extended and profound work commences with an inquiry into the relations subsisting between the individual and society. Proceeding by observation and analysis, the author studies the development of the consciousness of the individual, and his study bears at once upon that aspect of consciousness which relates to purely individual existence and upon that which relates to the society, great or small, to which the individual belongs. Occasionally the author makes contributions, as new as they are interesting, to the psychology of the child, and proves himself the same skilful observer in finding identical or analogous movements in different phases of conscious life. By the original, profound, and penetrating use which he makes of the psychological and genetic method, he has really cleared up the notions which must be used in the study of this question, and thereby made much progress toward its solution. . . . "

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3. Methodology of Psychology. By Assistant Professor Mead.

4. Psychology in Relation to Philosophy. By Assistant Professor Angell.

II. HISTORY OF PHILOSOPHY.

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Head Professor DEWEY gives a continuous two years' course, one year being theoretical, discussing respectively, the logical basis and method of ethics and its psychological and sociological aspects; the other historical, dealing not only with ethical reflection, but also with the pre-reflective evolution of morality. The historical course will be given in 1896–7. Professor LADD, of Yale, will give a seminary in the summer of '96. Professor SMALL, of the Sociological Department, offers courses in Social Ethics.

IV. LOGIC.

Head Professor DEWEY is giving a continuous two years' course. That of 1895–6 is historical; that of '96–'97 will develop the theory. Assistant Professor MEAD gives a course in the development of the concepts of matter and motion.

SUMMER QUARTER.

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